# Silicon Valley Everywhere? Startup Accelerators and the Geography of Entrepreneurship

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**Abstract**

Entrepreneurial entry reflects a fundamental tradeoff between strategic complementarities within local ecosystems and competition for scarce resources. This paper examines how this tradeoff shapes regional entrepreneurs' entry decisions. We exploit the staggered rollout of the Creative Destruction Lab (CDL), a global mentoring-based accelerator that introduced 27 streams across 10 cities between 2012 and 2022. To address endogenous program placement, we construct a novel instrument based on academic collaboration networks between business schools and the CDL's founding institution, the University of Toronto. We show that sector-specific launches reshape the composition of local entrepreneurship by reallocating startup formation toward treated sectors. Entry responses are weakened when local spillover potential is low, consistent with a complementarity-driven mechanism. Investor capital does not respond, resulting in intensified competition for financing. Motivated by these patterns, we develop a structural model that organizes the underlying mechanisms and provides suggestive policy counterfactuals. We document substantial heterogeneity in responses across sectors, with some targeted sectors experiencing costly crowding. Overall, the model suggests that generic policies may coordinate a broader base of participants more effectively than narrow targeting.

**JEL:** L26, O38, O31, R11, R58, L53, G24
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# 1 Introduction

Entrepreneurial entry is a primary driver of innovation and long-run economic growth (Romer (1990),Aghion and Howitt (1990)). Motivated by the local benefits of high-growth entrepreneurship (Moretti (2010)), governments increasingly rely on place-based innovation policies such as technology hubs, science parks, and startup accelerators. However, these interventions present policymakers with a rather complex problem: should they provide broad support to all technology firms, or "pick winners" by targeting specific industries? Furthermore, how are these choices mediated by local fundamentals?

Sectoral targeting can generate large gains when it leverages strategic complementarities. If entrepreneurs benefit from clustering with others in the same sector, then coordinating entry toward particular industries can create multiplier effects through knowledge spillovers, labor pooling, and networks (Jaffe et al., 1993; Glaeser et al., 1992; Duranton and Puga, 2004; Moretti, 2012). At the same time, targeting can impose costs. If policies pull founders into sectors where their fit is weaker, or into markets where scarce resources (capital, talent, customers) are already congested, then targeting may dilute average quality or create misallocation rather than sustained growth (Glaeser and Gottlieb, 2008; Akcigit and Kerr, 2018).

This paper aims to provide new evidence to the debates and questions by studying startup accelerators, an expanding policy tool that remains understudied in general-equilibrium policy design. We view accelerators as operating primarily through belief coordination rather than direct changes in fundamentals: by publicly committing to a region and an industry, they create a focal point that can strengthen local complementarities–facilitating connections among talent, ideas, and investors–without necessarily altering underlying fundamentals and capital supply. We exploit the staggered expansion of the Creative Destruction Lab (CDL), which launched 27 sector-specific streams across 10 cities between 2012 and 2022. CDL provides a clean setting because its sector-specific streams generate within-city, across-industry variation, and because it operates as a mentoring-based non-profit rather than a direct investor, allowing us to isolate coordination effects from capital shocks.

We develop a framework in which the entry decision reflects a tradeoff between complementarities and competition. On the one hand, ecosystem density raises the expected returns to entry through spillovers and network formation. On the other hand, the expected entry of other entrepreneurs intensifies competition for scarce resources, reducing each firm's chance of securing financing or reaching customers, thus lower expected return to entry ex-ante. Lastly, whether an accelerator launch expands entrepreneurial activity or mainly

reallocates would vary with local conditions. For example, whether existing human capital and sector density amplify complementarities or whether limited resources become the binding constraint.

Guided by this framework, our analysis asks three industry-targeting questions. First, do new industry streams expand local entry overall or primarily reallocate founders toward treated sectors? Second, does targeting specific industries generate costly crowding—either by increasing financing competition or by inducing mismatched entry that dilutes average quality? Third, how do local ecosystem features mediate the effectiveness of industry targeting, and what targeting strategy maximizes aggregate entry without exacerbating misallocation? By answering these questions, we evaluate when industry-focused accelerator expansions can move regions toward higher-activity equilibria and when they simply redistribute entrepreneurial effort across sectors.

The CDL setting is compelling and suitable for tackling the empirical questions. First, it creates the necessary variation needed for understanding entry choices (sectors and cities), while holding the underlying program design largely homogeneous. The common program blueprint and governance model across sites sharpens identification and makes it easier to map estimated effects to specific mechanisms in our structural model. Specifically, CDL operates through sector-specific streams (e.g., AI, health, quantum, energy) in each host city, which creates within-city, across-industry variations of treatment. This structure allows us to distinguish whether a new entry represents a genuine expansion of entrepreneurial activity or a reallocation of founders across sectors within the same region, speaking to the misallocation channel.

Moreover, CDL's institutional design lines up closely with the mechanisms we seek to separate. CDL is a not-for-profit program that does not itself invest capital, in contrast to many U.S. accelerators that operate as equity investors. As a result, CDL primarily affects potential entrepreneurs' beliefs towards fundamentals like mentoring, network access, and information exchange, rather than large direct financing shocks. More importantly, we observe the full list of CDL applicants and match them to Crunchbase. This allows us to isolate entry decisions that are directly motivated by the desire to apply to CDL and obtain its program-specific benefits. By focusing on the entry of *non-CDL applicants*, this setting allows us to understand how expectations about ecosystem-relevant fundamentals, rather than direct support from the accelerator, shape entrepreneurs' entry, which directly speaks to the complementarity channel we proposed.

We utilize both the reduced-form methods and structural estimation in our analysis, with the latter providing counterfactual analysis in policy design and implications. The main

challenge for empirical estimation is that accelerators endogenously locate in regions with strong entrepreneurial prospects, confounding causal effects with preexisting local advantages. Moreover, the types of streams launched depend on local conditions. For example, CDL introduced an "ocean" stream in Halifax, a city by the Atlantic Ocean, but not in landlocked Calgary. We address these concerns using an event-study design that exploits the staggered and exogenous timing of CDL launches, as well as an instrumental variable approach. We estimate effects using three complementary strategies: (i) difference-in-differences (DiD) across sectors within cities, (ii) triple-differences incorporating untreated cities, and (iii) city-level regressions with an instrumental variable approach, testing whether CDL entry expands overall city-level entrepreneurial activity. For robustness, we use three estimators for our event study design: the Two-way Fixed Effect estimator, the Callaway-Sant'Anna estimator, and the Sun & Abraham estimator.

Furthermore, CDL's institutional features allow us to deploy a novel instrumental variable to improve the quality of identification. Through conversations with program staff about CDL's expansion process, we learned that the decision to open a new stream hinge primarily on the willingness of a local business school to host the program, a process that typically unfolds over months or years and is not tightly linked to short-run entrepreneurial conditions. Because each site requires a local academic partner, CDL's geographic expansion is naturally linked to pre-existing academic networks rather than contemporaneous startup dynamics. We use OpenAlex data to obtain academic publications coauthored by University of Toronto faculty and researchers at institutions in potential host cities, restricted to business and management–related journals. This measure captures the strength of academic ties between Toronto and prospective sites. The assumption is that while academic connectivity between University of Toronto faculty and institutions in a focal city predicts CDL entry, it should not directly influence contemporaneous firm founding or financing in that city through any channel other than CDL entry. The instrument thus provides a valid source of exogenous variation for identifying the causal impact of accelerator entry on local entrepreneurship and financing.

Our empirical analysis yields five main findings.

First, accelerator launches increase entrepreneurial entry beyond accelerator participants and applicants, consistent with complementarity-driven spillovers. Each launch is associated with up to 60 additional startups. Using log entry, we show that it represents roughly a 10% increase. The effect remains economically and statistically meaningful even after excluding CDL applicants, at approximately 50 additional startups per launch.

Second, stream-specific launches induce within-city sectoral reallocation toward treated

sectors, with no evidence of cross-city reallocation. Using a city–industry–year panel and an event-study design with high-dimensional fixed effects, treated sectors experience approximately 8 additional startups relative to other sectors in the same city, a 27% increase in log terms. These results persist when excluding CDL applicants, with a slightly smaller magnitude of 7. A triple-differences specification yields a similar pattern: the entry gap between treated and untreated sectors within treated cities is about 8 firms larger than the corresponding gap in untreated cities (or 7, excluding applicants).

Third, we find no aggregate increase in total capital raised in treated sectors or host cities, suggesting that the short-run supply of capital is relatively inelastic. Instead, financing becomes more dispersed: the number of firms receiving at least one disclosed-amount financing round rises by about three per year after a stream launch, and the increase is substantially larger when including undisclosed rounds. This pattern is consistent with investors' "spray-and-pray" allocation strategy (Ewens et al. (2018)), and provides empirical support for the competition channel in our framework. Importantly, it also supports the view that CDL launches are not direct financing shock.

Fourth, we examine whether entry responses depend on the strength of local complementarities. We exploit two sources of variation: (i) the COVID-19 pandemic as an exogenous disruption to ecosystem interaction, which plausibly attenuates the returns to strategic complementarity; and (ii) the local share of CDL applicants as a proxy for latent spillover potential. Consistent with this mechanism, CDL launches during the pandemic or in cities with low local applicant share generate little to no additional entrepreneurial entry. In these settings, the estimated effect falls to a statistically insignificant 2-4 startups per launch.[1] These results support the model's core assumption that the expected return to entry is increasing in regional complementarities.

Fifth, we provide suggestive evidence that sector-targeted launches might be costly by comparing broad "prime" entry to sector-specific stream launches. CDL prime sites are generalist in scope and admit science-based startups across sectors, whereas non-prime streams target specific technologies (AI, health, ocean, energy, etc.). In the subset of cities that first launched a prime site and later added streams (Toronto, Halifax, Calgary, and Vancouver), prime entry is associated with a larger increase in city-wide firm formation than subsequent stream launches. Cross-sectional comparisons of post-launch cohorts further suggest that startups founded after prime entry exhibit stronger performance than those founded after stream-specific launches, relative to their respective peers. While these patterns are not fully

[1] CDL launched 12 streams across eight locations from 2020 to 2022, excluding one stream that specifically targets COVID-related ventures.

causal, they align with the idea that general platforms may coordinate ecosystem-wide entry more effectively than narrowly targeted interventions.

While this reduced-form evidence documents substantial heterogeneity across industries, the empirical analysis cannot separately identify the strength of complementarities from competition effects, or determine the optimal allocation of accelerator resources across sectors. We therefore complement the empirical analysis with a structural model of entrepreneurial entry that embeds both forces.[2]

We develop a structural model of startup entry following Aguirregabiria and Mira (2007). The model features strategic interactions through agglomeration spillovers and distinguishes between two types of treatment effects: a uniform effect of generic accelerators that applies equally across all sectors, and sector-specific parameters for targeted programs that vary by industry. We estimate the model using Nested Pseudo-Likelihood and find similar average treatment effects for both program types, but with substantial heterogeneity in targeted effects across sectors. Using these estimates, we conduct counterfactual simulations holding the total number of policy interventions fixed at observed levels. The results reveal that converting all interventions to generic accelerators generates significantly more firm entry than converting all to sector-specific programs. Despite some sectors exhibiting stronger responses to targeting, the coverage advantage of generic programs dominates.

The structural model yields three key insights for optimal industry targeting. First, we estimate strong within-sector complementarities ($\hat{\gamma}_{\text{within}} = 2.85$) alongside modest negative cross-sector externalities ($\hat{\gamma}_{\text{cross}} = -0.078$), confirming that agglomeration effects operate primarily within industries rather than across them. This finding provides a rationale for sector-specific targeting: launching an AI stream benefits AI entrepreneurs substantially while imposing only modest costs on other sectors in the same city. Second, we find substantial heterogeneity in treatment effects across industries. Health ($\hat{\tau} = 0.36$), AI ($\hat{\tau} = 0.22$), and Energy ($\hat{\tau} = 0.14$) experience significant positive effects from CDL streams, while Fintech ($\hat{\tau} = -0.21$) shows a negative effect—potentially reflecting increased scrutiny or crowding in already-competitive markets. This heterogeneity implies large gains from targeting: deploying accelerator resources in high-responsiveness sectors generates substantially more entry than uniform allocation. Third, counterfactual analysis reveals that spillover effects account for approximately 22 percentage points of observed entry probability—nearly 30% of the baseline level—while doubling financing would increase entry by only 0.4 percentage points.

[2]The structural model presented in this version is still a preliminary and stylized framework, and we are not using it to infer any welfare consequences at this point. In 6.7, we laid out a couple directions of model extensions, some of which we are currently working on.

This finding suggests that capital constraints are not the binding factor for entrepreneurial entry; rather, the coordination of expectations and ecosystem complementarities drives entry decisions.

This paper makes three contributions to existing literature. First, we provide causal evidence on how sector-specific launches affect the composition of local entrepreneurship, addressing a key limitation of existing studies that focus primarily on aggregate effects or participating firms. By documenting both expansion and reallocation, we show that targeting decisions have real consequences for which industries grow in a region.

Second, we show that the effectiveness of an accelerator launch in stimulating local entrepreneurial entry depends critically on the extent of underlying capacity for spillovers (the size of the entrepreneurial ecosystem) and its interaction with local fundamentals (infrastructure, education, human capital, etc.).

Third, we develop and estimate a structural model that decomposes the complementarity-competition tradeoff and enables counterfactual evaluation of alternative targeting strategies. The model moves beyond program evaluation ("does this program work?") to program optimization ("which industries should we target?"). Our analysis reveals that the effectiveness of industry targeting on entrepreneurial entry depends critically on sector-specific parameters that vary substantially across industries–information that policymakers can use to improve accelerator deployment.

Our findings speak directly to the design of place-based innovation policy. The conventional approach treats accelerator placement as a regional development problem: which cities should receive support? Our analysis adds an orthogonal dimension: *which industries should be targeted within each city?* The answer is not obvious. Targeting high-growth sectors like AI may seem natural, but if these sectors already have strong complementarities and active ecosystems, the marginal impact of an accelerator may be smaller than in sectors with latent but uncoordinated potential. Our structural estimates provide a framework for making these comparisons and allocating accelerator resources to maximize entrepreneurial entry.

The remainder of the paper proceeds as follows. Section 3 describes CDL, the data, and key definitions. Section 4 presents the empirical strategy. Section 5 reports reduced-form evidence on reallocation, expansion, and financing competition. Section 6 develops the model and links its implications to the heterogeneity tests, and Section 7 concludes.

## 2 Related Literature

This paper contributes to several bodies of literature. Our work sits at the intersection of entrepreneurial finance, economic geography, and innovation economics, providing novel causal evidence on how startup accelerators affect local entrepreneurial activity.

A growing body of work studies the geographic clustering of entrepreneurial activity and the factors that drive regional differences in startup formation and success. Pioneering work by Saxenian (1996) documented the importance of regional networks and institutional factors in explaining Silicon Valley's dominance, while Glaeser et al. (2015) provides systematic evidence on the role of human capital, industrial composition, and local amenities in determining entrepreneurial activity across U.S. cities. Lerner (2013) discusses the limits of innovation policy in promoting entrepreneurship, arguing that poorly designed programs risk misallocating resources and failing to generate additional innovation. More recent work by Guzman and Stern (2020) develops comprehensive measures of entrepreneurial quality and performance across regions, revealing substantial heterogeneity in both quantity and quality of entrepreneurial activity.

The role of venture capital in shaping entrepreneurial ecosystems has received considerable attention. Samila and Sorenson (2011) demonstrates that increases in local venture capital supply lead to greater entrepreneurial activity, while Lerner (2000) shows that venture capital is significantly more potent than R&D in stimulating innovation. However, Chen et al. (2009) finds that venture capital's impact depends critically on the pre-existing entrepreneurial infrastructure, suggesting that the effects of new intermediaries may vary across regional contexts.

The rapid increase in the number of startup accelerators has generated increasing academic interest in understanding their role in entrepreneurial ecosystems. Early work by Miller and Bound (2011) documented the growth of "startup factories" globally, while Fehder and Hochberg (2014) provides the first systematic evidence that accelerator programs increase local entrepreneurial financing. Their analysis of program launches across U.S. metropolitan areas finds positive effect,s on early-stage funding.

Gonzalez-Uribe and Leatherbee (2018) use a regression discontinuity design around Startup Chile's application cutoffs, showing significant impact on new venture performance. However, these studies focus primarily on direct effects on participating firms rather than ecosystem-wide spillovers. The mechanisms through which accelerators affect startups have also received attention. Hallen et al. (2020) synthesize existing evidence and propose that accelerators work primarily through reducing information asymmetries and providing social

capital connections. Dempwolf et al. (2014) emphasizes the role of mentorship and networking. Nejad (2024) develops a structural model to study different mechanisms through which mentorship improves entrepreneurial outcomes in an accelerator setting, and Sariri (2025) examines how advice from angel investors and venture capitalists within an accelerator program affect start-up performance.

Our work also contributes to the extensive literature on venture capital and entrepreneurial finance. Hellmann and Puri (2002) demonstrates that venture capital backing accelerates product development and market entry, and that VC-backed firms grow faster and are more likely to go public. Nanda and Rhodes-Kropf (2013) highlight how fluctuations in capital supply over investment cycles shape the type and innovativeness of startups that enter. Ewens et al. (2018) document and provide a framework to understand the shift in VC investment strategy toward a "spray and pray" approach. They show that this adoption disproportionately favors innovations where information about future potential is revealed quickly and cheaply, reducing relative investment in complex technologies with costly and less informative early signals.

The geographic dimension of venture capital has been explored by Cumming and Dai (2010), who find that local VC presence significantly affects regional entrepreneurial activity. Lerner et al. (2018) document the globalization of angel investing across countries, showing how cross-border capital flows expand entrepreneurial finance but also depend on institutional quality and local ecosystems. Agrawal et al. (2015) analyze geography and social networks in crowdfunding, showing that spatial and social proximity influence early-stage financing and the timing of entrepreneurial investment decisions. Recent work has examined whether venture capital expands or reallocates entrepreneurial resources. Bernstein et al. (2019) studies the impact of an influx of capital into the venture capital industry on the financing of early-stage innovation. Similarly, Howell (2017) uses variation in SBIR grant timing to show that R&D subsidies increase innovation without merely displacing private investment.

Our findings relate to the broader literature on place-based innovation policies and their effectiveness. Glaeser et al. (2010) provides a theoretical framework for evaluating place-based policies, emphasizing the importance of distinguishing between policies that create value versus those that redistribute economic activity. Kline and Moretti (2014) develop a structured approach to assessing the consequences of place-based policies, highlighting the challenges in identifying net welfare effects. Chatterji et al. (2014) review academic work on the spatial concentration of entrepreneurship and innovation in the United States and discuss the economic consequences of clusters. Wallsten (2000) studies federal R&D subsidies

and finds little evidence of additionally, suggesting that such programs primarily crowd out private investment.

# 3 Data and Sample

## 3.1 Institutional Setting: Creative Destruction Lab

Creative Destruction Lab (CDL) is a global startup accelerator program founded at the University of Toronto's Rotman School of Management in 2012. CDL operates as a non-profit mentoring program specifically designed for early-stage, high-technology ventures. The program's unique structure makes it particularly well-suited for studying the causal effects of startup-VC intermediation on local entrepreneurial ecosystems.

CDL's expansion follows a distinctive pattern that creates the variation we exploit for identification. The program launches sector-specific "streams" (such as artificial intelligence, healthcare, quantum computing, or fintech) in different cities at different times. Each stream is hosted by a business school in the target city and focuses on startups in a particular technological domain. This staggered rollout across both geographic and industry dimensions provides the quasi-experimental variation necessary to identify causal effects.

Between 2012 and 2021, CDL expanded from its original Toronto location to 10 sites across Canada, the United States, Europe, and Australia, launching 27 specialized technology streams. Each stream operates as a 9-month program featuring structured mentoring sessions where experienced entrepreneurs, venture capitalists, and industry experts provide guidance to participating startups. The program concludes each session with mentor commitments to support specific ventures, creating a direct link between startup participation and access to capital networks. A full description of the CDL dataset is provided in Sariri et al. (2025)

## 3.2 Data Sources

Our analysis combines three primary data sources that together provide comprehensive coverage of entrepreneurial activity, venture capital flows, and CDL program operations.

### CDL Program Data

We obtain detailed information on CDL stream launches from the program's administrative records. This dataset includes the timing of each stream launch, the host city and business school, the industry focus of each stream, and participation records. The data covers all stream launches from 2012 through 2021, providing us with 27 stream-city entry events across 10 cities. For each stream launch, we record the following:

- Launch Timing: the first year a stream began accepting ventures in a focal city
- Geographic Information: Host city, country, and associated business school
- Industry classification: Focus of each stream (e.g., AI, healthcare, fintech)
- Participation: Applications and admission records for each stream over time.

To ensure an accurate estimation of the effect of the treatment, we exclude two launches that lasted only one year before being discontinued: the *Recovery* stream, which aimed at COVID-related recovery solutions, and the *Prime* stream in New York City, which only ran for one year. Validating the *Recovery* stream shows that this industry specification is extremely rare, with essentially zero entries on average. Similarly, excluding New York City from the sample does not materially affect the results. The utilization of these programs varies depending on whether we employ the city–industry–year panel or the city–year panel, and on the chosen specification, which we explain in more detail in the methodology section.

Figure 2 shows the geographic distribution of CDL sites and partner institutions.[3] Figure 3 illustrates the staggered introduction of CDL programs across cities and sectors. Figure 4 displays the average cohort size by location, revealing a steady expansion from roughly 20 admitted startups per cohort to more than 60 in later years. Panel (a) of Table 3 reports summary statistics for the CDL program at more granular levels. On average, each city-sector admits about 25 ventures every year. For cities operating multiple sectors, the average number of accepted ventures at the city level increases to approximately 57 per year. The average number of applicants is around 176, implying an acceptance rate of roughly 30%. We also observe substantial variation in the share of local applicants across different locations, ranging from less than 10 percent to as high as 78 percent. This variation is exploited in one of our mechanism test.

[3]The map reflects the network as of 2025, including newly established sites in Australia, Berlin, and Estonia, which are not part of our sample that ends in 2022.

### Crunchbase Data

We complement the CDL data with Crunchbase, a comprehensive database of startup companies and investment activity. Crunchbase provides detailed information on company formation, funding rounds, and business characteristics that allow us to construct measures of local entrepreneurial ecosystems. From Crunchbase, we extract firm-level data, including founding date and geographic location (city, region, country), industry classification, and financing round details. From geographic location, we construct the distance from a city to the closest CDL site. Then we map Crunchbase industry categories to standardized classifications that align with CDL's stream categories. This mapping allows us to identify which startups would be eligible for specific CDL streams based on their technological focus.

We restrict our sample to firm founding and financing activities from 2000 to 2022 to ensure enough time period pre- and post-treatment. Table 3 Panel (b) provides the summary statistics of ventures based in the focal cities. 14.3% of the firm has at least one round of positive financing from investors.[4] Overall, firms are more likely to be in the AI and Health streams compared to the other CDL streams.

We then aggregate the firm-level sample to a city-industry-year panel by aggregating the number of firms founded (total amount raised) in a given year. Table 3 Panel (c) and (d) provides the summary statistics for all cities and the focal cities, respectively. We restrict to city-industry pairs that have at least one firm entry or positive financing in a given year.[5] Overall, both the number of new firms and aggregate financing are extremely right-skewed, with a few cities generating a large amount of entry and attracting capital inflows.

### OpenAlex Research Publication Data

To construct our instrumental variable, we use comprehensive academic publication data from OpenAlex API, an open-access bibliographic database covering over 200 million scholarly works across all disciplines. OpenAlex provides structured metadata on publications, authors, institutions, and citation patterns. We focus specifically on publications in economics, business, and management journals to capture the professional networks most relevant to CDL's expansion strategy. We employ a list of 100+ top-tier business journals spanning multiple fields, including general management, economics, finance, marketing, operations, entrepreneurship, and international business.

[4] We exclude non-monetary rounds like non-equity assistance and post-ipo rounds.

[5] We've also constructed a balanced panel where we impute all missing values as zero. Whether using a balanced or unbalanced panel does not materially affect our results.

An advantage of using publications as the primary proxy for academic connectivity is that it alleviates concerns that business research is reacting quickly to industry waves (such as ESG, real estate, or AI). Because there is typically a significant lag between when a study is conducted and when it is published, a paper appearing in 2020 may actually reflect connections formed as early as 2016. This lag helps avoid conflating academic connectivity with contemporaneous shocks occurring in 2020, thereby better satisfying the exclusion restriction.

Figure A.1 illustrates the dynamics of business-school paper collaboration between the University of Toronto and institutions in cities worldwide, a key component for our IV. We focus on the five countries most frequently represented in the IV dataset. Notably, the top four—the United States, Canada, the United Kingdom, and France—are precisely the focal countries in which a CDL site was launched during our sample period. On average, the connection intensity, the average number of co-authored papers with researchers in a city in a given year, is the highest in Canada. This is reasonable given that U of T is a Canadian institution. Table 3 Panel (e) further shows the summary statistics for some additional components of our IV, including the cumulative number of papers and citations. In our test, we deploy both cumulative number of papers and time-varying instruments. The next section describes the construction of these variables in detail.

## 3.3 Key Variable Construction

### Outcome Variables

- *Firm Entry:* The number of new startup companies founded in each city-industry-year or city-year combination, as recorded in Crunchbase
- *Log(Entry):* The logarithm of firm entry
- *Aggregate Financing:* The logarithm of total financing raised by startups within each city-industry-year or city-year pair, measured in USD
- *Number of Firms Received Financing:* The number of startups received investor financing within each city-industry-year or city-year pair, measured in USD

### Treatment Variables

- *Treated City-Stream × Post:* An indicator equal to one for all periods after the launch of a given stream in a focal city, and zero for city–stream pairs that are not yet treated or never treated. Each city–stream can only be launched once.

- *Treated City × Post:* An indicator equal to one for all periods after the **first** CDL launch in the city, and zero for cities that are not yet treated or never treated.[6]

- *Treatment Intensity:* An integer capturing the cumulative number of CDL launch events over time, to reflect that many cities experience multiple launches. Treatment intensity increases by one if at least one launch occurs in a given year. For example, Toronto reaches a treatment intensity of six by 2021.

## 3.4 Instrumental Variables

A primary empirical challenge in identifying the causal effects of startup accelerators on local entrepreneurial ecosystems is the endogenous location of such programs. Accelerators strategically choose cities and timing based on expected returns, local entrepreneurial potential, and pre-existing advantages that may themselves affect startup formation and venture capital flows. This endogeneity threatens the validity of difference-in-differences estimators, as treatment assignment is correlated with unobserved determinants of entrepreneurial outcomes.

To address this challenge, we construct a novel instrumental variable that exploits the institutional structure governing CDL's expansion decisions. Since all CDL programs must be hosted by a business school in the target city, expansion decisions are influenced by the academic networks and collaborative relationships between the University of Toronto, which is CDL's founding institution, and business schools in potential host cities. We leverage this institutional feature to construct instruments based on academic connectivity in business research between Toronto faculty and researchers at institutions in prospective CDL cities.

### Academic Connectivity Measures

For each potential CDL host city $c$ and time period $t$, we construct several measures of academic connectivity between the University of Toronto and institutions in target city $c$. First, we construct a time-invariant binary connection measure:

[6] In a stacked DiD specification, each treatment is grouped into a stack, which allows us to capture the average treatment effect of each launch if a city experienced more than one launch.

$$\text{Has_Academic_Connection}_c = \mathbf{1}[\text{Collaborations}_c > 0]$$

where $\text{Collaborations}_c$ represents the count of joint publications between University of Toronto's faculty and researchers at institutions in city $c$. We also measure the intensity of academic relationships using citation-weighted number of collaborations:

$$\text{Connection_Strength}_c = \sum_{p \in P_c} (1 + \ln(1 + \text{Citations}_p))$$

where $P_c$ denotes the set of collaborative publications between Toronto and city $c$, and $\text{Citations}_p$ is the citation count for paper $p$. The logarithmic form reduces the influence of extremely highly cited papers. We then construct a time-invariant instrument:

$$\text{Academic_Connectivity_Instrument}_c = \ln\left(\frac{\text{Connection_Strength}_c}{1 + \text{Distance}_c}\right)$$

where $\text{Distance}_c$ is the distance between Toronto and city $c$ (in kilometers).

We develop time-varying instruments that capture both cumulative and recent connectivity patterns:

$$\text{Time_Varying_Instrument}_{ct} = \text{Recent_Connection}_{ct} + \alpha \cdot \text{Cumulative_Connection}_{c,t}$$

where:

$$\text{Recent_Connection}_{ct} = \sum_{s=t-3}^{t-1} \sum_{p \in P_{cs}} (1 + \ln(1 + \text{Citations}_p))$$

$$\text{Cumulative_Strength}_{c,t} = \sum_{s=t_0}^{t-4} \sum_{p \in P_{cs}} (1 + \ln(1 + \text{Citations}_p))$$

The parameter $\alpha \in [0, 1]$ is the relative importance of historical collaborations, which we set to 0.5.

The validity of our instrumental variable strategy depends on two key assumptions:

**Relevance Condition** We use the fact that CDL expansion requires business school partnerships, and academic relationships facilitate these institutional arrangements. Our academic connectivity must predict CDL expansion decisions. We formally test relevance through both a cross-section test that shows how the academic connectivity in business-related subjects, between a focal city and the University of Toronto, is correlated with the likelihood of having a CDL site in the focal city (Table A.4) and also the first-stage regressions showing that our time-varying connectivity instrument also strongly predict CDL entry (all F-statistics larger than 10: Table **??**).

**Exclusion Restriction** Academic publication patterns between Toronto and potential host cities are not correlated to the conditions of the regional industry, and the relevance is about administrative/institutional capacity, not research content.

# 4 Methodology

## 4.1 Reallocation: Event-Study Framework

To identify the causal effects of Creative Destruction Lab (CDL) stream launches, we exploit their staggered rollout across cities. Because our Crunchbase sample ends in 2022, we restrict attention to CDL launches between 2012 and 2019 to ensure sufficient post-treatment observations. The "Prime" stream, which targets firms with general "technology" rather than an industry focus, is typically introduced before any sector-specific stream. We exclude all "Prime" streams from the analysis, as our objective is to study how industry-oriented launches shape local industry composition.

First, we compare treated and untreated sectors within the same city using traditional DiD with two-way fixed effects estimation, by focusing on cities with at least one CDL launch. We further deploy Callaway–Sant'Anna Estimator to pair observations at t0 and t1, as a robustness check. Second, we implement a triple-difference approach that incorporates never-treated cities as additional controls, absorbing both city-year and industry-year shocks, as well as city-industry cross-sectional variations.

### Specification 1: Within-City Industry Comparison

Our baseline identification strategy exploits the variation in the timing of CDL stream launches across sectors within the same city. We restrict the sample to treated cities that

host at least one CDL stream during our observation period, and estimate the following difference-in-differences specification at the city-industry-year level:

$$Y_{cit} = \beta \cdot \text{Treated Industry}_i \times \text{Post}_{cit} + \gamma_{ci} + \tau_{ct} + \varepsilon_{cit} \tag{1}$$

where $Y_{cit}$ represents our outcome variables (firm entry or financing) for city $c$, industry $i$, in year $t$. The variable $\text{TreatedIndustry}_i \times \text{Post}_{cit}$ equals one for all periods after the launch of a given stream targeting the industry in city $c$ after year $t$. We include city-industry fixed effects ($\gamma_{ci}$) to control for time-invariant differences across city-industry pairs, and city-year fixed effects ($\tau_{ct}$) to account for city-specific temporal shocks. Standard errors are clustered at the city-industry level.

The coefficient $\beta$ captures the change in outcomes for treated sectors relative to untreated sectors within a city after the launch of the CDL stream. This specification controls for any cross-sectional differences between cities in terms of their industry composition (for example, Halifax has more fishing-related businesses while Calgary does not), as well as city-level shifts in the entrepreneurial environment (for example, city-level startup expansion). With these fixed effects in place, the coefficient likely captures the reallocation of entrepreneurs across sectors.

The identifying assumption is that treated and control sectors within the same city would have followed parallel trends in the absence of CDL intervention. Figure 6 (a) presents estimators using the Callaway-Sant'Anna estimator, with Panel (b) showing alternative TWFE and Sun & Abraham estimators, both of which indicate that the parallel trends assumption is not violated.

**Specification 2: Triple Difference with Control Cities**

To address potential concerns about sector-specific trends and shocks that might coincide with CDL launches, we expand our analysis to include cities that never host CDL streams as additional controls. This allows us to implement a triple-difference strategy with high-dimensional fixed effects:

$$Y_{cit} = \beta \cdot \text{Treated City}_c \times \text{Treated Industry}_i \times \text{Post}_{cit} + \gamma_{ci} + \delta_{ct} + \theta_{it} + \varepsilon_{cit} \tag{2}$$

This specification includes city-industry fixed effects ($\gamma_{ci}$), city-year fixed effects ($\delta_{ct}$),

and industry-year fixed effects ($\theta_{it}$). The coefficient $\beta$ now represents the DiD estimate for treated versus untreated sectors within treated cities, net of the corresponding difference in untreated cities during the same time periods.

This approach accounts for both city-specific temporal shocks (such as local economic cycles or policy changes) and sector-specific temporal trends (such as technological developments or market conditions affecting particular sectors globally). The coefficient captures the extent to which entrepreneurs reallocate towards treated sectors within a focal city, using such trends in never-treated cities as a counterfactual.

The identification assumption is that the interaction between city and industry trends would be similar across treated and untreated cities in the absence of CDL intervention. Figure A.2 presents two estimators: TWFE and Sun & Abraham, all of which indicate that the parallel trends assumption is satisfied.

## 4.2 Expansion: Instrumental Variable Approach

Going beyond reallocation, we are interested in whether the CDL launch would expand city-level entrepreneurship activities. To test this hypothesis, instead of focusing on shocks at the city-industry level, we observe multiple shocks (CDL launches) at the city level over time. According to Figure 3, a city can experience from 1 to 6 treatments from 2012 to 2021.

### Specification 3: Aggregate City-Level Analysis

To test whether CDL stream launches generate positive spillovers to the broader local entrepreneurial ecosystem or reallocate activity across sectors, we aggregate outcomes to the city level and estimate:

$$Y_{ct} = \beta \cdot Cumulative_Number_of_Launches_{ct} + \alpha \cdot Controls_{ct} + \gamma_c + \tau_t + \varepsilon_{ct} \qquad (3)$$

where $Y_{ct}$ represents total firm entry or aggregate financing across all sectors in city $c$ during year $t$. We include city fixed effects ($\gamma_c$) and year fixed effects ($\tau_t$), with standard errors clustered at the city level.

To address endogenous program placement, we estimate two-stage least squares models for Specification 3, instrumenting for CDL launches using time-varying academic connectivity

between the University of Toronto and potential host cities. The coefficient $\beta$ measures whether cities experiencing CDL stream launches see changes in aggregate entrepreneurial activity relative to control cities. A positive and significant $\beta$ would suggest that CDL creates net positive spillovers to the local ecosystem, while an insignificant coefficient would be consistent with reallocation of entrepreneurial activity across sectors without expanding the overall entrepreneurial pie.

# 5 Reduced-form Results

This section interprets the results of estimating the specifications in equations 1, 2, and 3.

## 5.1 Reallocation

### Firm Entry

We first look at within-city, whether there are certain reallocation of entrepreneurial entry towards targeted sector. We control for city-industry and city-year level unobservables using fixed effects. Table 4 shows the *firm entry* dynamics around CDL stream launch. Panel A presents the results of estimating specification 1 using the Callaway–Sant'Anna estimator, and Panel B presents the results of estimating 2 with multiple levels of interacted fixed effects. Both estimators show an increase in the number of new firms in treated city–industry pairs relative to untreated ones, with similar magnitudes. Results suggest reallocation of entrepreneurial activities across sectors once overall city-level entrepreneurial activity is accounted for. Columns (2)–(3) report the effects after excluding all CDL applicants and attendees. The estimators remain significant, with slightly lower magnitude, suggesting that the CDL launch not only encourages firm creation for the purpose of participating in the incubator program but also generates broader spillover effects beyond the hub, affecting the entire local entrepreneurial ecosystem. To better interpret the results, Panel A columns (4)-(6) present log estimators. On average, the number of new firms created increased by around 27% after the launch.

In addition to reallocation across sectors, we also examine whether reallocation occurs across cities. Figure 7 presents this analysis. Panel (a) plots the evolution of firm entry around a CDL stream launch for treated and untreated sectors within the focal city, assuming that counterfactual trends follow those of the corresponding sectors in never-treated cities. Untreated sectors exhibit a decreasing trend around the launch, while treated sectors show

a sharp response in the event year, suggesting strong evidence of crowding out non-treated sector's entrepreneurial activities.

Panel (b) compares firm entry into a city's treated industry to entry into never-treated industries within the same city, assuming that trends in never-treated industries serve as a counterfactual for the city's average attractiveness to entrepreneurs. Under this assumption, any divergence captures a change in the city's overall attractiveness around the shock. We find no economically meaningful evidence of reallocation across cities.

### Financing

Table 5 shows response of the number of firms that received financing (Columns (1)-(2)) and the aggregate funding invested in all startups (Columns (3)-(4)), using samples including and excluding CDL participants. For the number of firms receiving financing , we estimate effects using only financing rounds with disclosed amounts [7]. While columns (3) and (4) show that the aggregate amount of post-launch investment is not statistically different from zero, we observe a significant increase in the number of firms receiving financing. This pattern implies that financing is spread across more firms, potentially generating competition and crowding-out effects.

## 5.2 Expansion

To test whether CDL stream launches expand the overall local entrepreneurial pie as simply opposed to reallocating activity across sectors within the city, we aggregate outcomes to the city–year level and estimate equation 3 using 2SLS with instrumental variables.

We start our analysis by introducing tests for the instrument's relevance condition. Table A.4 reports OLS regressions where the dependent variable is an indicator for whether a city hosts a CDL site. The independent variables are three measures of academic connectivity between the University of Toronto and institutions in the focal city: (i) an indicator for any prior collaboration (*Has_Academic_Connection*), (ii) a continuous index based on citation-weighted co-authorship counts (*Connection_Strength*), and (iii) a log-scaled index that adjusts for Toronto–city distance (*Academic_Connectivity_Instrument*). Across specifications without and with country fixed effects, the estimates are positive and statistically significant for all measures. Economically, cities with Toronto ties are more likely to host CDL, and stronger ties predict a higher probability of hosting. This finding aligns with the

[7] Using all financing rounds will triple the magnitude, with not material effects on economic significance

program's institutional requirement that streams be hosted by a local business school and supports the use of academic connectivity as an instrument of CDL stream launch.

We next address the endogenous timing and placement of CDL streams by estimating two-stage versions of our event study specifications. We instrument the treatment indicator with time-varying academic connectivity between Toronto and potential host cities, constructed from OpenAlex publication ties in business-related fields. Tables 6 and 7 present two-stage least squares estimates. Panel A reports results for Cities with CDL sites using a set of city-year economics control variables; Panel B restricts the sample to Canadian Cities; Panel C restricts the sample to Canadian Cities that have enough city-year economics control variables. Across all panels, the first stage shows strong predictive power of academic connectivity for CDL launches, with Kleibergen–Paap Wald statistics well above conventional thresholds, supporting instrument relevance.

Tables 6 show that firm entry in focal cities increases following the launch, with an expansion of approximately 55 firms when excluding CDL applicants—corresponding to about a 10% increase. These results are consistent with findings in the previous section: while the percentage magnitude is smaller, the overall effect remains positive. Taken together, our results suggest that CDL programs stimulate new entrepreneurial activity at the city level, with treated sectors disproportionately attracting more new entrants. Table 7 Columns (3) - (4) confirms that the overall financing effect remains insignificant with negative coefficients for some specifications. The result further confirms that overall CDL don't have much impact on investors.

To summarize, accelerator entry leads to a substantial increase in startup formation, with local entry spillovers extending beyond program participants. At the same time, accelerators do not appear to expand the overall supply of investor capital, but they may encourage greater investor experimentation through a "spray-and-pray" approach. Our finding that more firms receive financing is consistent with this mechanism.

## 5.3 Mechanisms and Consequences

### Complementarities

After documenting both reallocation and expansion effects, we next examine whether firm entry responds to local spillover potential – an assumption embedded in our model. In the model, entry becomes more attractive when other firms in the same sector enter, as geographic proximity facilitates knowledge diffusion, the formation of specialized labor pools,

and signaling benefits. If entry is driven by such regional complementarities, the effect of accelerator launches on firm entry should weaken when these complementarities are disrupted.

We implement two heterogeneity tests. First, we exploit the COVID-19 pandemic as an exogenous disruption to ecosystem interactions, which plausibly reduces the benefits of strategic complementarities. Second, we compare stream launches with a high versus low share of local applicants. The local composition of applicants is informative along multiple dimensions: it may reflect how closely a stream aligns with local comparative advantage, and it may also proxy for the strength of local spillover potential. While accelerator launches are unlikely to alter sectoral fundamentals directly, they can amplify spillovers by encouraging entrepreneurial entry. Accordingly, we hypothesize that launches with a higher proportion of local applicants would lead to stronger entry responses beyond the accelerator itself.

Tables 8 and 9 report the results of these two heterogeneity tests. In Table 8, columns (1) and (3) show entry responses for launches with a high share of local applicants, while columns (2) and (4) report results for launches with a low local share. Launches with greater local participation are significantly more likely to induce firm entry, both economically and statistically, and these findings are robust to using log-transformed outcomes. Table 9 applies the same event-study specification as in Table 4, restricting the sample to launches occurring during the COVID period. Again, we find no statistically meaningful entry outcomes and even negative coefficients when we use log(entry). Taken together, these results support the model's assumption that the returns to entry increase with regional complementarities.

### Potential Crowding Effects Behind Sectoral Targeting

The next question we are trying to tackle is whether targeting specific industries generates costly crowding – either by increasing financing competition or by inducing mismatched entry that dilutes average quality.

We first provide suggestive evidence of potential crowding effects on financing competition. Specifically, we examine financing outcomes across different firm ages. This focus is motivated by the hypothesis that incubators and accelerators may redirect investor attention and resources toward younger, early-stage firms.

Table A.3 presents the results. Comparing Panel A and Panel B, we find that firms aged 4–7 face the fiercest competition: a larger number of such firms receive funding, but the aggregate amount of financing to this sub-group declines. In contrast, older firms (aged 8 and above) face a lower probability of receiving funding; however, conditional on receiving financing, they benefit from greater allocations given that the aggregate level of funding

remains fixed. Finally, the youngest firms (aged 0–3) experience increases in both the number of firms receiving financing and the total amount of financing. These patterns are consistent with the competition mechanism in our model: the program might redirect investor attention from older firms toward younger ones, making entrepreneurial entry more attractive but harming mid-stage firms.

Second, CDL provides a great setting for studying sectoral targeting, because it has both generic launches that target all science-based tech startups as well as launches that target specific sectors, even in the same city but in different years. This allows us to simply test the entry effects of generic versus targeted launches, as well as comparing the quality of incubator-induced startups.

Table 10 reports the results. Panel A presents city-year entry responses for technology and non-technology firms following prime-site launches, while Panel B reports the corresponding entry responses for stream launches. Because a city may experience multiple stream launches, Panel B is estimated using a stacked difference-in-differences framework. Moreover, since all CDL-targeted sectors are high-tech, firms are aggregated by their technology classification (tech versus non-tech) rather than by finer sector definitions.

Comparing the magnitudes of the estimates in columns (1)-(3) across both panels, we find that in cities that first launched a prime site and subsequently added streams (Toronto, Halifax, Calgary, and Vancouver), prime-site launches are associated with larger increases in city-wide tech firm formation than later stream launches. Columns (4) through (6) show that prime-site launches are also associated with larger positive coefficients for non-tech firm entry, although these estimates are not statistically significant. In contrast, stream launches have little impact on non-tech firm entry, both economically and statistically. The results suggest that generic launches are more likely to induce entrepreneurial entry.

Next, we examine how accelerator-induced startups perform in the years following the two types of policies. We conduct a cross-sectional analysis of firm outcomes over the first three years after founding. A firm is defined as treated if it is founded after the introduction of a CDL launch in the relevant city–industry. To distinguish between prime and non-prime launches and to avoid overlapping treatment windows, we impose the following sample restrictions. For prime launches, we retain firms founded between 2009 and 2015 in Toronto and firms founded between 2014 and 2019 in Halifax. Firms founded outside these windows and within three years before and after the stream launches are used to identify the effects of non-prime (stream) launches. This design allows us to cleanly separate the two policy regimes.

We examine three dimensions of firm performance: (i) failure, (ii) financing outcomes, and (iii) extreme success. All specifications include founding-team controls – team size, prior experience, ethnicity, gender, and education – as well as city fixed effects, city-industry fixed effects whenever available, and founding-year fixed effects. Variable construction is detailed in Appendices A.3.

Table 11 presents the results. Panel A reports performance outcomes for firms induced by prime launches, while Panel B reports outcomes for firms induced by stream launches. Column (1) measures failure, defined as an indicator for whether the firm's website is inactive as of July 2025.[8] Columns (2) through (5) capture the amount and quality of financing, and Column (6) captures extreme outcomes.

Comparing estimates across panels, we find that firms induced by prime launches are less likely to fail, raise fewer but more frequently funded rounds, attract higher-quality financing led by venture capital investors, and are more likely to exit through IPOs or acquisitions.[9]

Nonetheless, cross-sectional comparisons of post-launch cohorts suggest that startups founded after prime entry exhibit stronger performance than those founded after stream-specific launches, relative to their respective peers. While these patterns are not fully causal, they speak to the potential that targeted launches may induce mismatched entry, diluting average startup quality.

# 6 Structural Model of Firm Entry

We develop a structural model of firm entry to quantify the effects of accelerator programs and conduct policy counterfactuals. The model distinguishes between two types of accelerator interventions: *prime* (generic) programs that support all sectors equally, and *targeted* programs that focus on specific industries. This distinction allows us to evaluate the trade-off between broad-based versus focused intervention strategies.

[8] This measure is collected in July 2025.

[9] Though we fully acknowledge that these results should be interpreted with caution: prime launches occurred earlier in the sample, and some outcomes may not fully materialize within the observation window for non-prime firms. Also, prime launches target a broader set of firms than sector-specific stream launches; thus, no implementation of city-industry fixed effect.

## 6.1 Model Environment

Consider a potential entrant in region $r$, sector $s$, at time $t$. The firm decides whether to enter the market based on expected profitability, which depends on local market conditions, agglomeration spillovers, and policy interventions.

**Timing.** At the beginning of each period $t$, firms observe: (i) prior venture capital activity in the local market, $F_{rs,t-1}$; (ii) entry patterns of other firms in the same city-year; and (iii) policy interventions (accelerator launches). Firms then simultaneously decide whether to enter.

**Payoffs.** The expected payoff from entry is:

$$U_{rst} = \underbrace{\delta_s}_{\text{sector FE}} + \underbrace{\beta F_{rs,t-1}}_{\text{financing}} + \underbrace{\gamma_{\text{cross}} \Phi^{-s}_{rst}}_{\text{cross-sector}} + \underbrace{\gamma_{\text{within}} \Phi^{s}_{rst}}_{\text{within-sector}} + \underbrace{\tau^{\text{prime}} D^{\text{prime}}_{rst} + \tau^{\text{targeted}}_{s} D^{\text{targeted}}_{rst}}_{\text{policy effects}} + \varepsilon_{rst} \quad (4)$$

where:

- $\delta_s$ captures sector-specific baseline entry propensities
- $F_{rs,t-1}$ is the log of total venture capital raised in region-sector $(r, s)$ in period $t-1$
- $\Phi^{-s}_{rst} = \sum_{s' \neq s} P_{rs't}$ is the expected number of entrants in *other* sectors in the same city-year (cross-sector spillovers)
- $\Phi^{s}_{rst} = P_{rst}$ is the own entry probability (within-sector agglomeration)
- $D^{\text{prime}}_{rst} \in \{0, 1\}$ indicates whether a *prime* (generic) accelerator is active
- $D^{\text{targeted}}_{rst} \in \{0, 1\}$ indicates whether a *targeted* (sector-specific) program is active
- $\varepsilon_{rst}$ is an i.i.d. Type-I extreme value shock

**Key Distinction: Prime vs. Targeted Programs.** The model captures a fundamental asymmetry between the two policy instruments:

1. **Prime (Generic) Accelerators:** These programs support startups across *all* sectors equally. When a city launches a prime accelerator, every sector in that city-year receives the same treatment effect $\tau^{\text{prime}}$. We therefore estimate a *single* $\tau^{\text{prime}}$ parameter.

2. **Targeted Programs:** These interventions focus on *specific* sectors (e.g., fintech, healthtech). The effect may vary by sector due to differences in program design, mentorship quality, or sector-specific market frictions. We estimate *sector-specific* parameters $\tau_s^{\text{targeted}}$ for each sector $s$ that receives targeted treatment.

This specification reflects the institutional reality: prime accelerators cast a wide net, accepting startups regardless of industry, while targeted programs specialize in particular verticals.

## 6.2 Equilibrium

Given the logit error structure, the probability of entry satisfies:

$$P_{rst} = \frac{\exp(V_{rst})}{1 + \exp(V_{rst})} = \Lambda(V_{rst}) \tag{5}$$

where $V_{rst}$ is the deterministic component of utility (equation 4 without $\varepsilon_{rst}$) and $\Lambda(\cdot)$ is the logistic CDF.

The spillover terms $\Phi_{rst}^{-s}$ and $\Phi_{rst}^{s}$ create strategic interdependence: each firm's entry decision depends on the expected entry decisions of others. An equilibrium is a fixed point in the space of conditional choice probabilities (CCPs):

$$P_{rst}^* = \Lambda\left(\delta_s + \beta F_{rs,t-1} + \gamma_{\text{cross}} \sum_{s' \neq s} P_{rs't}^* + \gamma_{\text{within}} P_{rst}^* + \tau^{\text{prime}} D_{rst}^{\text{prime}} + \tau_s^{\text{targeted}} D_{rst}^{\text{targeted}}\right) \tag{6}$$

Following the literature on games with strategic complementarities (Brock and Durlauf, 2001), existence of equilibrium is guaranteed. When $\gamma_{\text{within}}$ is sufficiently small relative to the slope of $\Lambda(\cdot)$, the equilibrium is unique.

## 6.3 Identification

The structural parameters are identified from distinct sources of variation:

**Sector Fixed Effects ($\delta_s$).** These are identified from cross-sectional variation in average entry rates across sectors, conditional on other observables.

**Financing Effect ($\beta$).** Identified from within-region-sector variation in entry rates following changes in local venture capital activity. The lagged structure ($F_{rs,t-1}$) mitigates concerns about reverse causality.

**Spillover Parameters ($\gamma_{\text{cross}}, \gamma_{\text{within}}$).** Identification exploits the timing structure and cross-sectional variation. Consider two cities with identical financing and policy environments but different historical entry patterns. The model predicts different current entry rates through the spillover channel. The functional form of the logit model provides additional identification from the nonlinearity of the equilibrium mapping (Aguirregabiria and Mira, 2007).

**Prime Treatment Effect ($\tau^{\text{prime}}$).** This *single* parameter is identified from:

- Temporal variation: comparing entry rates before and after prime accelerator launches
- Cross-city variation: comparing treated cities to untreated cities
- The "all sectors" nature: when a prime accelerator launches, *every* sector in that city-year is treated, providing identifying variation across the full cross-section of sectors

**Targeted Treatment Effects ($\tau_s^{\text{targeted}}$).** These sector-specific parameters are identified from:

- Within-city-year variation: comparing targeted sectors to non-targeted sectors in the same city-year
- Temporal variation: before/after comparisons within treated city-sectors
- The sector-specific nature: targeted programs create differential treatment across sectors within the same city-year, enabling identification of heterogeneous effects

## 6.4 Estimation

We estimate the model using Nested Pseudo-Likelihood (NPL), following Aguirregabiria and Mira (2007). The NPL algorithm iterates between:

1. **Policy Iteration:** Given current CCP estimates $\hat{P}^{(k)}$, compute spillover terms:

$$\hat{\Phi}_{rst}^{-s,(k)} = \sum_{s' \neq s} \hat{P}_{rs't}^{(k)} \tag{7}$$

$$\hat{\Phi}_{rst}^{s,(k)} = \hat{P}_{rst}^{(k)} \tag{8}$$

2. **Maximization:** Estimate parameters by maximum likelihood:

$$\hat{\theta}^{(k+1)} = \arg\max_{\theta} \sum_{r,s,t} \left[ Y_{rst} \log \Lambda(V_{rst}(\theta, \hat{\Phi}^{(k)})) + (1 - Y_{rst}) \log(1 - \Lambda(V_{rst}(\theta, \hat{\Phi}^{(k)}))) \right] \tag{9}$$

   where $Y_{rst} \in \{0, 1\}$ is the observed entry indicator.

3. **CCP Update:** Update CCPs using estimated parameters:

$$\hat{P}_{rst}^{(k+1)} = \Lambda(V_{rst}(\hat{\theta}^{(k+1)}, \hat{\Phi}^{(k)})) \tag{10}$$

4. **Convergence:** Iterate until $\max_{r,s,t} |\hat{P}_{rst}^{(k+1)} - \hat{P}_{rst}^{(k)}| < \epsilon$ for tolerance $\epsilon = 10^{-5}$.

We initialize CCPs using observed entry frequencies smoothed toward 0.5 to avoid boundary issues. Standard errors are computed via bootstrap at the city-year level (200 replications), which accounts for correlation in unobservables within markets.

## 6.5 Estimation Results

Table 1 presents the parameter estimates.

**Core Parameters.** The financing effect ($\beta = 0.080$) is positive and significant: a one-unit increase in log VC activity increases entry utility. The within-sector agglomeration effect ($\gamma_{\text{within}} = 2.495$) is large and positive, indicating strong positive feedback—entry begets entry. The cross-sector spillover ($\gamma_{\text{cross}} = -0.079$) is negative but small, suggesting mild competition for resources across sectors within a city.

**Treatment Effects.** The prime treatment effect ($\tau^{\text{prime}} = 0.517$) represents the utility gain from generic accelerator access, affecting all sectors equally. The average targeted effect ($\bar{\tau}^{\text{targeted}} = 0.532$) is similar in magnitude, but with substantial heterogeneity across sectors.

Table 1: Structural Model Estimates

| | Estimate | Std. Error | $z$-stat |
|---|---|---|---|
| *Panel A: Core Parameters* | | | |
| $\beta$ (Financing) | 0.080 | (0.024) | $3.33^{***}$ |
| $\gamma_{\text{cross}}$ (Cross-sector spillover) | $-0.079$ | (0.031) | $-2.55^{**}$ |
| $\gamma_{\text{within}}$ (Within-sector agglomeration) | 2.495 | (0.412) | $6.06^{***}$ |
| *Panel B: Prime Treatment Effect* | | | |
| $\tau^{\text{prime}}$ (Generic accelerator) | 0.517 | (0.156) | $3.31^{***}$ |
| *Panel C: Targeted Treatment Effects (Selected Sectors)* | | | |
| $\tau^{\text{targeted}}_{\text{Fintech}}$ | 0.842 | (0.298) | $2.83^{***}$ |
| $\tau^{\text{targeted}}_{\text{Healthtech}}$ | 0.623 | (0.315) | $1.98^{**}$ |
| $\tau^{\text{targeted}}_{\text{Edtech}}$ | 0.451 | (0.287) | 1.57 |
| $\tau^{\text{targeted}}_{\text{Enterprise}}$ | 0.389 | (0.342) | 1.14 |
| Average $\tau^{\text{targeted}}$ | 0.532 | — | — |
| Observations | | 5,984 | |
| Log-likelihood | | $-2{,}847.3$ | |
| NPL Iterations | | 12 | |

*Notes:* Nested Pseudo-Likelihood estimates. Standard errors (in parentheses) computed via city-year block bootstrap with 200 replications. Sector fixed effects estimated but not reported. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Fintech shows the strongest response ($\tau^{\text{targeted}}_{\text{Fintech}} = 0.842$), while enterprise software shows weaker effects ($\tau^{\text{targeted}}_{\text{Enterprise}} = 0.389$).

## 6.6 Counterfactual Policy Analysis

We use the estimated model to evaluate alternative policy regimes. The key question is: *given a fixed number of accelerator interventions, should policymakers deploy generic (prime) or sector-specific (targeted) programs?*

### Counterfactual Design

We simulate four scenarios, holding the *number of policy interventions* (accelerator launches) fixed at $N = N^{\text{prime}} + N^{\text{targeted}}$:

1. **No Treatment:** Remove all accelerator effects ($\tau^{\text{prime}} = \tau_s^{\text{targeted}} = 0$). This provides the counterfactual baseline.
2. **Baseline:** Actual observed policy mix ($N^{\text{prime}}$ prime shocks + $N^{\text{targeted}}$ targeted shocks).
3. **All Prime:** Convert all $N$ shocks to generic accelerators. Each shock now affects *all sectors* in its city-year with effect $\tau^{\text{prime}}$.
4. **All Targeted:** Convert all $N$ shocks to sector-specific programs:
   - Original targeted shocks: remain targeted to their original sectors
   - Original prime shocks: converted to targeted shocks in the sector with *highest lagged entry probability* in that city (reflecting that programs would target sectors with demonstrated entrepreneurial activity)

**Key Insight: Coverage vs. Intensity Trade-off.** With fixed $N$ total shocks, the scenarios differ in treatment *coverage*:

- **All Prime:** Each of $N$ shocks affects all $S$ sectors. Total treated sector-observations $= N \times S$ (broad coverage).
- **All Targeted:** Each of $N$ shocks affects exactly 1 sector. Total treated sector-observations $= N$ (narrow coverage, higher intensity per treated unit).

The counterfactual thus isolates the trade-off: Does the broader reach of generic programs ($N \times S$ treated observations at $\tau^{\text{prime}}$ each) generate more entry than the focused intensity of targeted programs ($N$ treated observations at $\tau_s^{\text{targeted}}$ each)?

### Simulation Procedure

We simulate entry dynamics forward from the first shock year:

1. **Initialize:** Set entry probabilities to pre-shock period averages for each city-sector pair.
2. **For each year $t$ from first shock year to final year:**
   (a) Compute spillover terms using lagged entry probabilities
   (b) Compute deterministic utility $V_{rst}$ for each observation
   (c) Apply treatment effects based on the counterfactual scenario
   (d) Compute entry probabilities: $P_{rst} = \Lambda(V_{rst})$
   (e) Simulate entry realizations: $Y_{rst} \sim \text{Bernoulli}(P_{rst})$
   (f) Update history for next period's spillovers
3. **Aggregate:** Sum entries across city-sector-years for each scenario.

The dynamic simulation captures the spillover feedback: entries in period $t$ affect entry probabilities in $t + 1$ through the agglomeration channel.

### Counterfactual Results

Table 2 presents the counterfactual results.

**Main Finding.** With the same 92 policy interventions, the "All Prime" scenario generates **11 more entries** than "All Targeted" (1,303 vs. 1,291). The key driver is *treatment coverage*: All Prime treats 682 sector-observations versus only 92 for All Targeted.

Table 2: Counterfactual Policy Comparison (Fixed $N = 92$ Total Shocks)

| Scenario | Treated Sector-Obs | Expected Entries | Treatment Effect |
|---|---|---|---|
| No Treatment | 0 | 1,291 | — |
| Baseline (observed mix) | 498 | 1,302 | +11 |
| All Prime (generic) | 682 | 1,303 | +12 |
| All Targeted (sector-specific) | 92 | 1,291 | +0 |
| **All Prime − All Targeted** | +590 | — | **+11** |

*Notes:* Counterfactual simulation holding total shocks fixed at $N = 92$ (26 prime + 66 targeted in baseline). Treatment Effect = expected entries relative to No Treatment. "Treated Sector-Obs" counts the number of sector-observations receiving treatment effect. All Prime treats $N \times S$ sector-observations; All Targeted treats exactly $N$.

**Decomposition.** The advantage of prime accelerators operates through the *extensive margin*:

- Each prime shock affects all $S = 17$ sectors in a city-year
- Each targeted shock affects exactly 1 sector
- With $N = 92$ total shocks: All Prime covers 682 sector-observations; All Targeted covers exactly 92
- Despite similar per-observation effects ($\tau^{\text{prime}} \approx \bar{\tau}^{\text{targeted}}$), the 7× difference in coverage drives the result

The "All Targeted" scenario produces the *same* expected entries as "No Treatment" because, while it treats 92 sector-observations intensively, this narrow coverage cannot overcome the baseline entry rate.

Figure 1 visualizes the entry dynamics under each scenario.

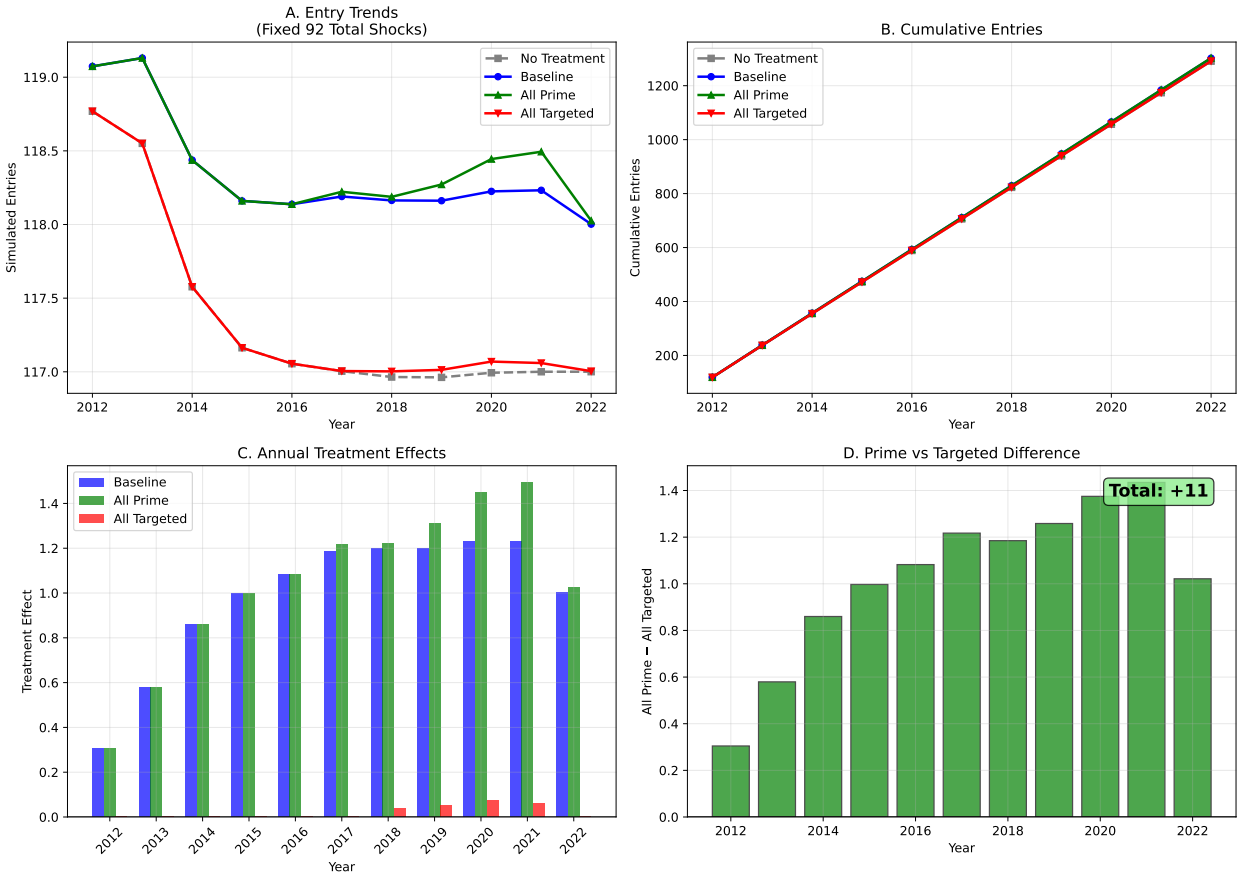



Figure 1: Counterfactual Policy Simulation Results

*Notes:* Panel A shows annual entry counts by policy scenario. Panel B shows cumulative entries. Panel C shows annual treatment effects relative to the No Treatment baseline. Panel D shows the annual difference between All Prime and All Targeted scenarios. Green bars indicate years where Prime outperforms Targeted; red bars indicate the opposite.

## 6.7 Discussion and Model Extension

The counterfactual analysis shows that *generic accelerators dominate targeted programs* for maximizing total startup entry. It provides a quantitative benchmark: policymakers choosing between broad and narrow intervention strategies should recognize that broad-based programs offer substantially greater entry-generating potential per intervention.

However, this conclusion warrants several caveats:

1. **Quantity vs. Quality:** Our analysis focuses on entry *counts*. If targeted programs select higher-quality startups or generate larger firms, the welfare comparison may differ.

2. **Sector-Specific Objectives:** Policymakers may value entry in specific sectors (e.g., cleantech for climate goals). Targeted programs can achieve sector-specific objectives that generic programs cannot.

3. **Diminishing Returns:** At scale, generic programs may face capacity constraints or declining marginal effects. Our model assumes constant treatment effects.

4. **Selection:** The counterfactual assumes treatment effects are portable across contexts. If targeted programs are deployed in sectors with high latent demand, converting them to generic programs may yield smaller effects than estimated.

## 6.8 Future Extension of the Model

**Entrepreneur heterogeneity (quality of entry)** The current model assumes homogeneous entrepreneurs within sectors. This abstraction prevents the model from fully capturing selection and quality of entry, which are central to the interpretation of sectoral targeting policies. A natural extension is to introduce heterogeneity in entrepreneurial quality or project type, allowing the model to quantify how accelerators affect not only the quantity but also the composition of entrants.

**Investor behavior and financing response** The model currently treats the supply of investor capital as predetermined and does not allow for strategic investor responses to accelerator entry. This assumption is motivated by the reduced-form evidence showing little aggregate expansion in capital supply, but it abstracts from potentially important heterogeneity in investor attention across firm age or stage. An extension is to relax this assumption

by allowing investor behavior to respond endogenously. For example, new startups might receive more attention from investors than late staged firms following accelerator launches.

**Local fundamentals and sector interaction** The present model abstracts from interactions between sector-specific entry decisions and local fundamentals beyond past financing and entry density. An important extension is to allow sector-level fundamentals–such as human capital intensity, university research strength, or local industry maturity–to interact with accelerator targeting. Incorporating these interactions would allow the model to better rationalize why targeting is effective in some sectors but not others, and to generate more nuanced policy implications.

**Model Identification** Finally, an important direction for future work is to strengthen identification by explicitly linking structural parameters to quasi-experimental variation in the data.

# 7 Conclusion

This paper provides causal evidence on how startup accelerators—acting as coordination mechanisms—shape the landscape of local entrepreneurial ecosystems. By exploiting the staggered rollout of sector-specific streams within the Creative Destruction Lab (CDL), we distinguish between the expansion of the entrepreneurial frontier and the internal reallocation of startups across sectors. Our findings suggest that while accelerators effectively "grow the pie" of firm entry, they do so within a relatively inelastic financial environment, creating a complex trade-off between ecosystem density and resource competition.

Our analysis yields three primary insights into the mechanics of place-based innovation policy. First, we find clear evidence of an expansionary effect; an accelerator launch increases city-wide startup entry by approximately 10%, an effect that persists even when excluding direct program applicants. However, this is accompanied by significant sectoral reallocation, where entrepreneurs shift toward treated industries and fundamentally alter the local industrial composition over time. Second, in contrast to the surge in entry, we find no aggregate increase in the supply of risky capital. Instead, financing becomes more dispersed, suggesting intensified competition for a fixed pool of local resources. This confirms that CDL operates primarily as a belief-coordination device rather than a direct capital shock. Third, our structural model reveals that while within-sector complementarities are strong, the effec-

tiveness of industry targeting is highly heterogeneous. While sectors like AI and Health show robust responses, others face potential costly crowding, suggesting that generalist platforms may often coordinate a broader base of ecosystem participants more effectively than narrow targeting.

These results offer a cautionary yet constructive roadmap for the design of place-based policies. The success of an accelerator is deeply contingent on local fundamentals; when physical interaction is disrupted, or local spillover potential is low, the coordination power of these programs evaporates. Furthermore, our findings also speak to the potential misallocation risks behind sectoral targeting. Because accelerators expand the founder base without necessarily expanding the capital base, they risk inducing entry where average firm quality may be diluted by increased competition. These patterns suggest that the effectiveness of mentoring-based, place-based policies is tightly linked to local financial architecture. Moving regions to higher-activity equilibria likely requires pairing coordination-oriented interventions with measures that relax capital constraints, such as attracting outside investors or facilitating cross-border venture flows.

While we document the short-run dynamics of entry and financing, the long-run welfare implications remain an open question. Whether the reallocation of talent toward "favored" sectors leads to more productive regional specialization or creates fragile clusters prone to misallocation depends on the long-term performance of these cohorts. Future research should continue to investigate the survival and exit valuations of accelerator-induced startups to determine if these shifts in local industry composition translate into sustained, high-quality economic growth.

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Figure 2: **CDL Map.** This map shows the current map of CDL locations and the streams.

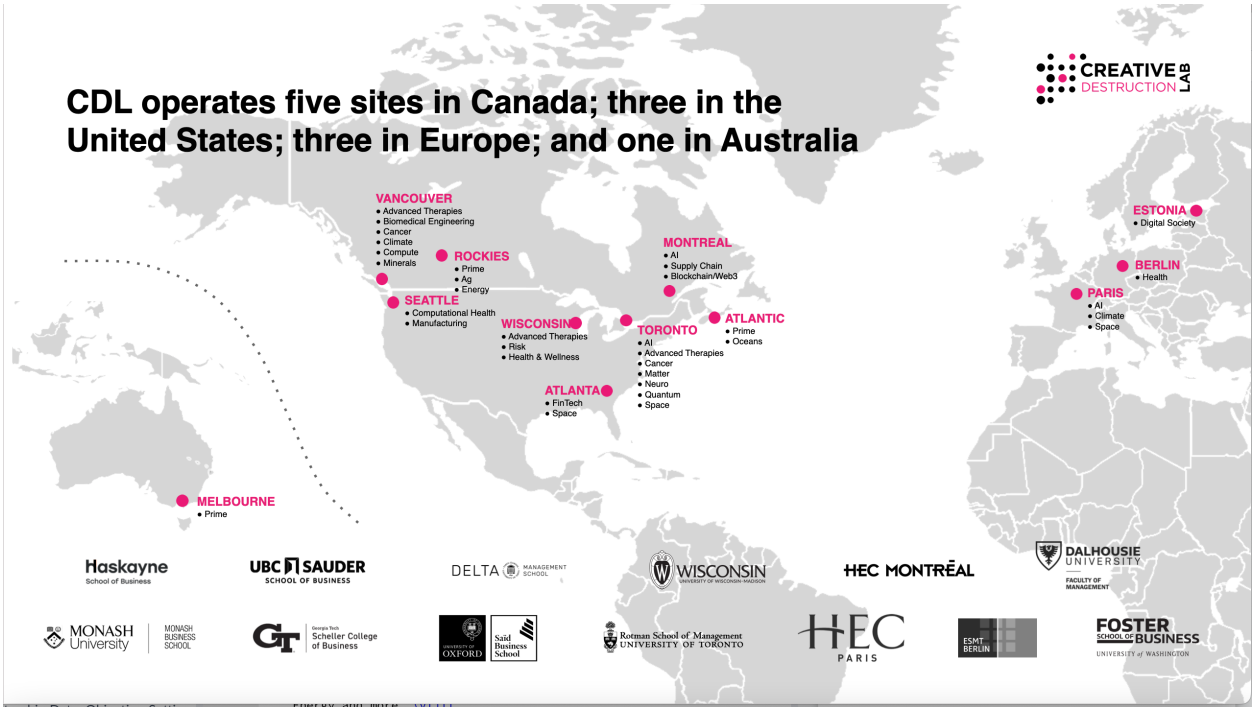


Figure 3: **CDL Launching Dynamics.** This figure shows the launch timing of each CDL city-stream pair.

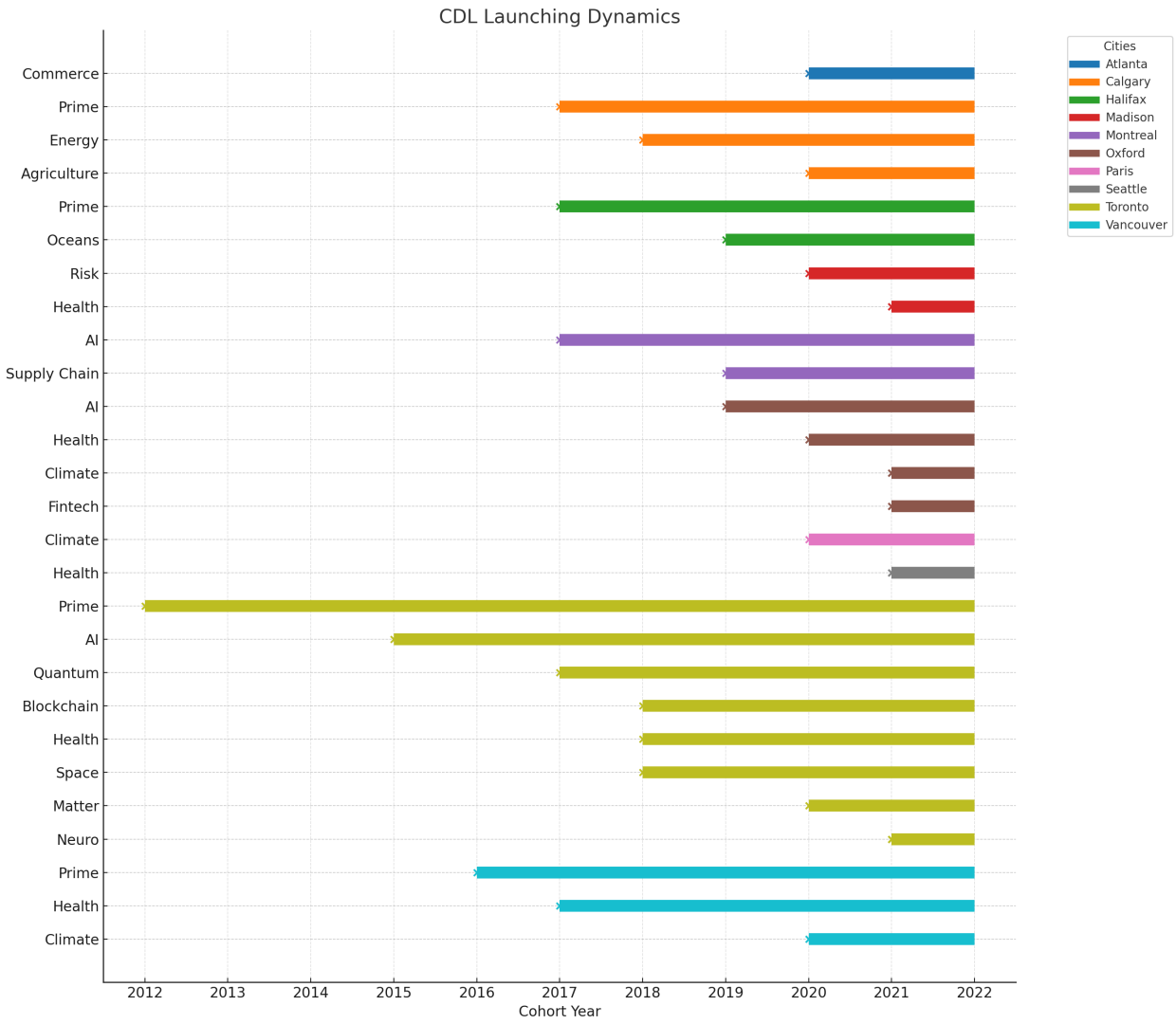


Figure 4: **Size of the CDL Program In Focal Cities.** This figure shows the average number of firms accepted into CDL across focal cities, from the first cohort year (2012) to the most recent cohort (2021) in our data.

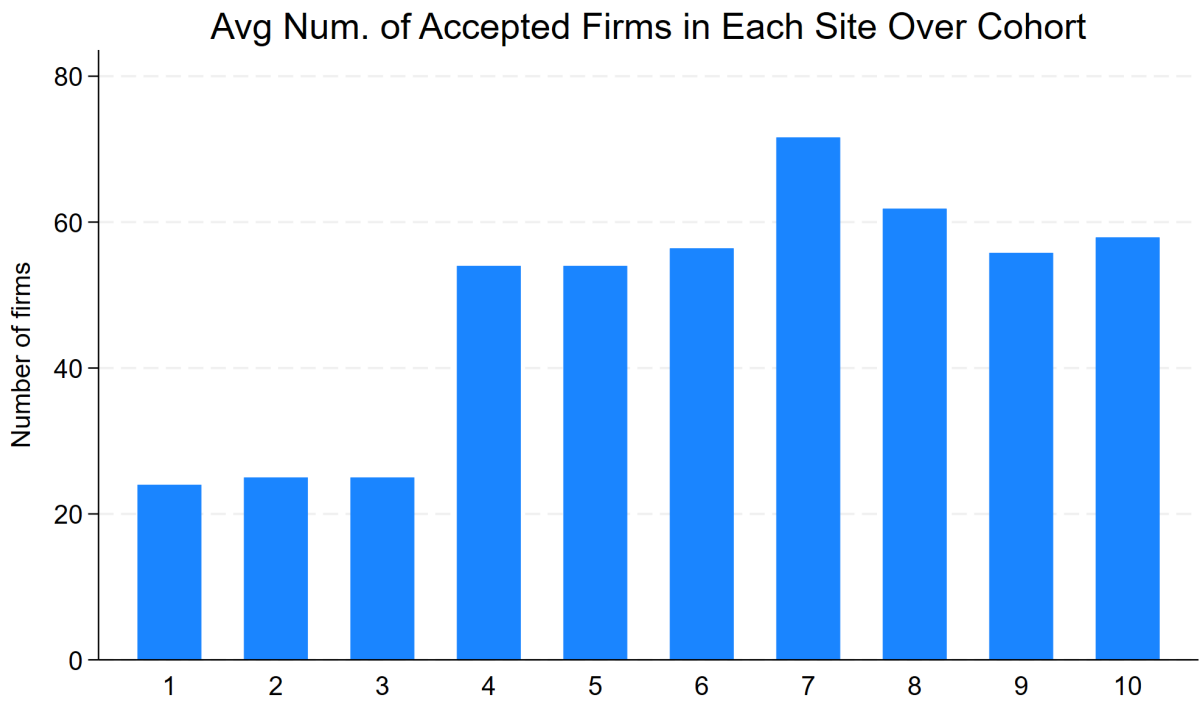
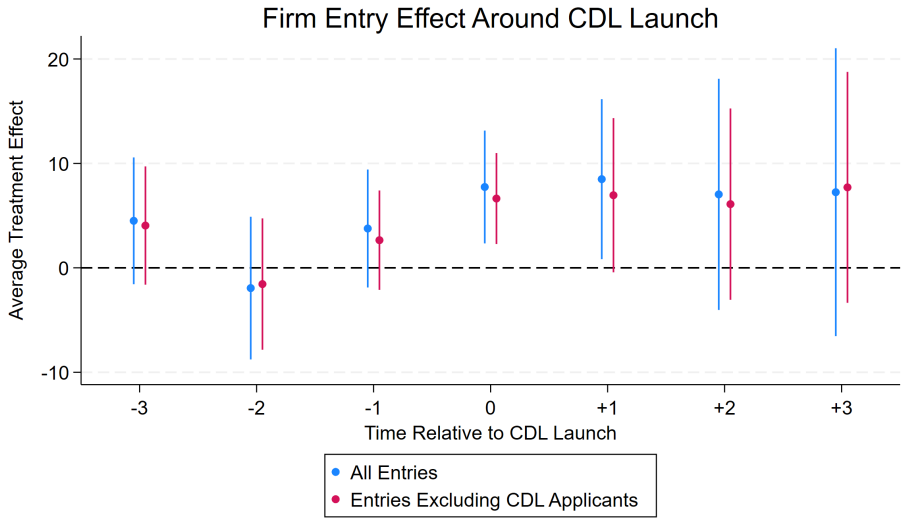
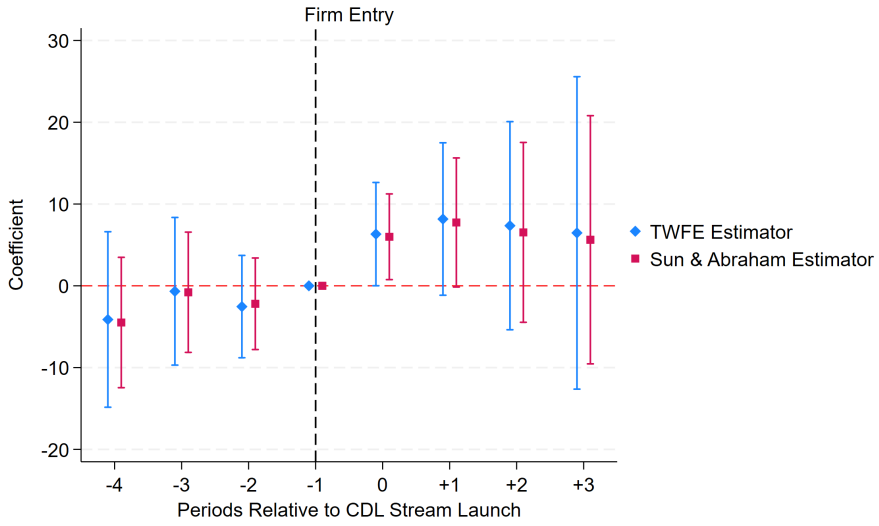



Figure 5

Figure 6: **City-Industry Market Dynamics Around New CDL Stream Launch: Focal Cities Only.** This figure shows the dynamics of firm entry and the amount of financing attracted around a new stream launch by CDL. We estimate a dynamic DID comparing the treated stream with the non-treated ones within the same city by following (1) and using TWFE, Sun & Abraham, and Callaway-Sant'Anna estimators. To allow for an adequate post-period, we restrict our analysis to launches before 2020 and control groups to never-treated or not-yet-treated city-industry pairs. The specification includes city × industry FEs and year FEs. The year before the event is omitted as the base year. Standard errors are clustered by city × industry.

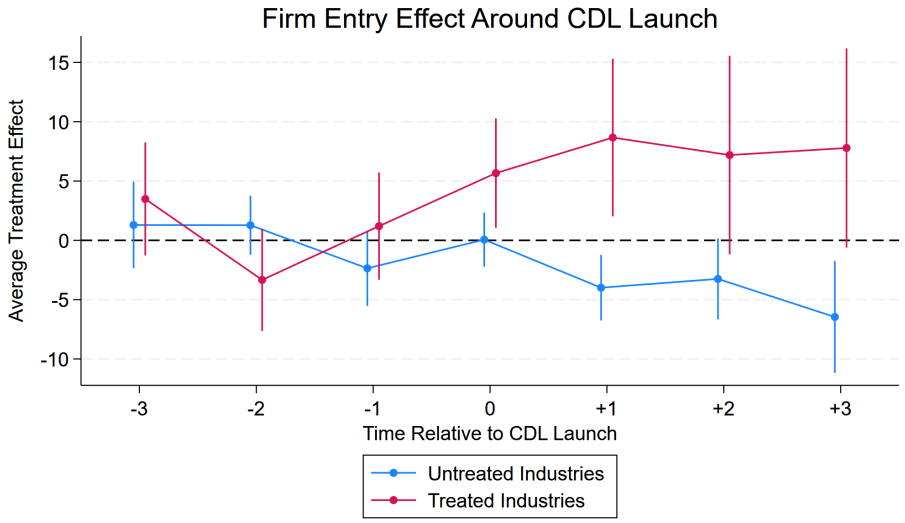



(a)

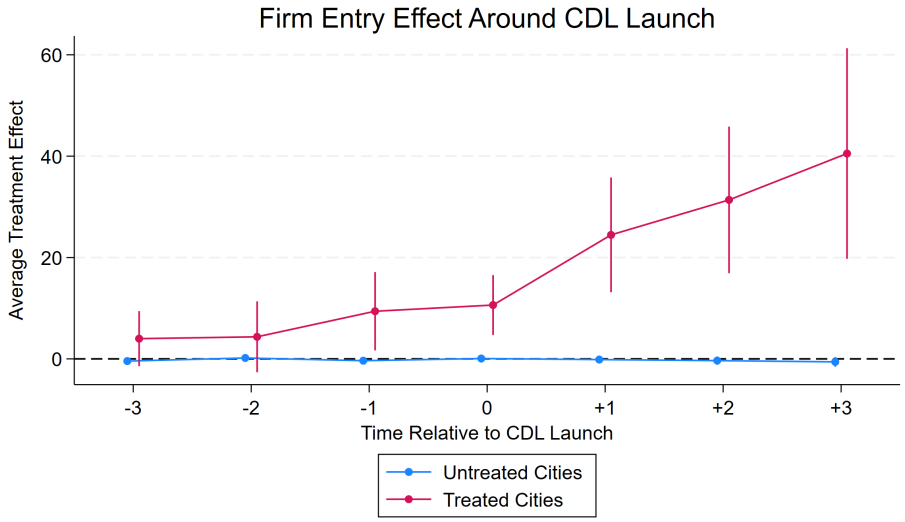



(b)

Figure 7: **Treated vs. Untreated Groups Dynamics Around New CDL Stream Launch.** The figure shows reallocation patterns across industries and cities around the CDL Stream Launch in Canada, by plotting the Callaway-Sant'Anna estimator. Figure (a) compares firm entry into an industry within the focal city to entry into the same industry in never-treated cities, assuming that trends in never-treated cities provide a valid counterfactual for that industry's average attractiveness to entrepreneurs. Any difference reflects a change in an industry's attractiveness around the shock. We present results separately for (1) treated industries and (2) never-treated industries. Similarly, Figure (b) compares firm entry into a city's treated industry to entry into never-treated industries within the same city, assuming that trends in never-treated industries serve as a counterfactual for the city's average attractiveness to entrepreneurs. Any difference reflects a change in a city's attractiveness around the shock. We present results separately for (1) treated cities and (2) never-treated cities. Standard errors are clustered by city × industry.




(a) Firms Reallocate Across Industries




(b) Firms Reallocate Across Cities

Table 3: **Comprehensive Summary Statistics**

(a) CDL Global Accelerator Program

| | count | mean | sd | p5 | p50 | p95 |
|---|---|---|---|---|---|---|
| Number of Accepted Ventures (Cohort-Site-Stream) | 94 | 24.766 | 10.264 | 18.000 | 24.000 | 44.000 |
| Number of Accepted Ventures (Cohort-Site) | 41 | 56.780 | 46.300 | 19.000 | 47.000 | 170.000 |
| Number of Applicants (Cohort-Site) | 42 | 175.905 | 197.756 | 19.000 | 114.500 | 422.000 |
| Share of Applicants from Local (Cohort-Site) | 41 | 0.380 | 0.232 | 0.083 | 0.333 | 0.778 |

(b) Venture Features (Focal Cities Only)

| | count | mean | sd | p5 | p50 | p95 |
|---|---|---|---|---|---|---|
| founding_year | 48783 | 2011.986 | 5.763 | 2001.000 | 2013.000 | 2020.000 |
| has_financing | 48783 | 0.143 | 0.350 | 0.000 | 0.000 | 1.000 |
| AI | 48783 | 0.113 | 0.316 | 0.000 | 0.000 | 1.000 |
| Agriculture | 48783 | 0.005 | 0.074 | 0.000 | 0.000 | 0.000 |
| Blockchain | 48783 | 0.013 | 0.113 | 0.000 | 0.000 | 0.000 |
| Climate | 48783 | 0.005 | 0.072 | 0.000 | 0.000 | 0.000 |
| Commerce | 48783 | 0.023 | 0.150 | 0.000 | 0.000 | 0.000 |
| Energy | 48783 | 0.036 | 0.186 | 0.000 | 0.000 | 0.000 |
| Fintech | 48783 | 0.067 | 0.251 | 0.000 | 0.000 | 1.000 |
| Health | 48783 | 0.104 | 0.306 | 0.000 | 0.000 | 1.000 |
| Matter | 48783 | 0.003 | 0.053 | 0.000 | 0.000 | 0.000 |
| Neuro | 48783 | 0.013 | 0.112 | 0.000 | 0.000 | 0.000 |
| Oceans | 48783 | 0.026 | 0.160 | 0.000 | 0.000 | 0.000 |
| Other | 48783 | 0.235 | 0.424 | 0.000 | 0.000 | 1.000 |
| Other_Hightech | 48783 | 0.328 | 0.469 | 0.000 | 0.000 | 1.000 |
| Quantum | 48783 | 0.000 | 0.013 | 0.000 | 0.000 | 0.000 |
| Recovery | 48783 | 0.000 | 0.010 | 0.000 | 0.000 | 0.000 |
| Risk | 48783 | 0.006 | 0.080 | 0.000 | 0.000 | 0.000 |
| Space | 48783 | 0.011 | 0.105 | 0.000 | 0.000 | 0.000 |
| Supply_Chain | 48783 | 0.011 | 0.102 | 0.000 | 0.000 | 0.000 |

Table 3: **Summary Statistics (Continued)**

(c) City-Industry Level: Full Sample

| | count | mean | sd | p5 | p50 | p95 |
|---|---|---|---|---|---|---|
| Num_New_Firm | 474318 | 2.878 | 12.240 | 1 | 1 | 8 |
| Founding_Year | 474318 | 2010.631 | 6.065 | 2001 | 2011 | 2020 |
| Agg_Financing_MilUSD | 107676 | 61.1 | 527.5 | 0.05 | 3 | 193.9 |
| Announced_Year | 107676 | 2015.348 | 5.306 | 2004 | 2016 | 2022 |

(d) City-Industry Level: Treated Cities

| | count | mean | sd | p5 | p50 | p95 |
|---|---|---|---|---|---|---|
| Num_New_Firm | 2842 | 19.979 | 37.790 | 1 | 6 | 94 |
| Founding_Year | 2842 | 2011.309 | 6.461 | 2001 | 2012 | 2021 |
| Agg_Financing_MilUSD | 1974 | 152.834 | 433.869 | 0.286 | 21.410 | 741.450 |
| Announced_Year | 1974 | 2013.926 | 5.971 | 2002 | 2015 | 2022 |

(e) Instrument Variable (Agg at Collaborator City)

| | count | mean | sd | p5 | p50 | p95 |
|---|---|---|---|---|---|---|
| papers published in past 3 years | 365 | 14.932 | 29.600 | 2.000 | 6.000 | 57.000 |
| citations received in past 3 years | 365 | 2367.608 | 6128.611 | 30.000 | 522.000 | 10186.000 |
| cumulative_papers | 365 | 43.586 | 90.854 | 2.000 | 17.000 | 178.000 |
| cumulative_citations | 365 | 8155.159 | 22849.255 | 44.000 | 1582.000 | 33080.000 |
| time_varying_instrument | 365 | 197.398 | 414.813 | 8.466 | 75.624 | 749.885 |

*Notes:* Panel A describes the size and variation of the CDL program. Panel B focuses on venture-level characteristics. Panels C and D provide city-industry level entry and financing aggregates. Panel E provides summary statistics for the basic components we used to construct the instrumental variables.

Table 4: **Firm Entry Dynamics Around CDL Stream Launch.** This table presents the causal estimates of CDL stream launches on firm entry at the city–industry level in a given year, using specification 1 (Panel A) and specification 2 (Panel B). Both the full sample and the sample excluding CDL applicants and attendees are used. Standard errors are clustered by city × industry.

**Panel A: Treated Cities**

| | Number of New Firms Founded | | | Log(Num New Firms) | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| ATT (CSDID) | 7.570** | 7.438** | 6.811** | 0.236** | 0.239** | 0.224** |
| | (3.826) | (3.673) | (3.105) | (0.109) | (0.104) | (0.103) |
| Sample | Full | Excluding Accepted | Excluding Applied | Full | Excluding Accepted | Excluding Applied |
| City × Industry FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 1245 | 1245 | 1245 | 1245 | 1245 | 1245 |

**Panel B: All Cities in Countries with CDL Sites**

| | Number of Firms New Firms Founded | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| SR | 8.235** | 7.835** | 7.208** |
| | (3.825) | (3.597) | (3.383) |
| MR | 6.126 | 7.016 | 6.519 |
| | (10.942) | (10.559) | (9.368) |
| Sample | Full | Excluding Accepted | Excluding Applied |
| City × Industry FE | Yes | Yes | Yes |
| Stream × Year FE | Yes | Yes | Yes |
| City × Year FE | Yes | Yes | Yes |
| Observations | 180647 | 180647 | 180647 |
| $R^2$ | 0.885 | 0.885 | 0.885 |

Table 5: **Competition Dynamics Around CDL Stream Launch.** This table reports the causal estimates of the CDL stream launch on the competition for financing between startups. We mainly look at two metrics: the number of firms received at least one round of financing in a given year around the event, conditional on firm age at the time it receives funding, and the total amount of funds raised by firms. These two proxies together should depict the level of competition for financing among local firms. The analysis uses the full sample and the sample excluding CDL participants. We estimate effects using financing rounds with disclosed amounts. Another test using all financing rounds reveals a 3-times magnitude with the same level of significance. Standard errors are clustered by city × industry.

**Panel A: Treated Cities**

| | Number of Firms Recieved Financing | | Log(Total_Amount_Raised) | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| ATT (CSDID) | 2.878** | 2.182* | -0.428 | -0.598 |
| | (1.199) | (1.158) | (0.457) | (0.447) |
| Sample | Full | Excluding Accepted | Full | Excluding Accepted |
| City × Industry FE | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes |
| Observations | 1211 | 1211 | 1136 | 1122 |

**Panel B: All Cities in Countries with CDL Sites**

| | Number of Firms Recieved Financing | | Log(Total_Amount_Raised) | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| SR | 3.356** | 2.477* | -0.153 | -0.222 |
| | (1.447) | (1.359) | (0.399) | (0.415) |
| MR | 3.998 | 2.175 | -0.470 | -0.509 |
| | (2.702) | (2.413) | (0.422) | (0.448) |
| Sample | Full | Excluding Accepted | Full | Excluding Accepted |
| City × Industry FE | Yes | Yes | Yes | Yes |
| Stream × Year FE | Yes | Yes | Yes | Yes |
| City × Year FE | Yes | Yes | Yes | Yes |
| Observations | 50382 | 50382 | 41263 | 41068 |
| $R^2$ | 0.809 | 0.809 | 0.714 | 0.713 |

$t$ statistics in brackets

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: **Does CDL Stream Launch Spur Aggregate-level of Entrepreneurship? IV Estimation.** This table uses a time-varying academic connectivity in business-related subjects, between institutions in a focal city and the University of Toronto, as an instrument for a CDL launch in the focal city. We use the instrument for two treatment variables, where *Has_CDL*=1 after the first treatment and *Treatment_Intensity*∈[1,6] accounts for the fact that certain cities have multiple launches over the time period. Each panel reports both the first stage, which tests the relevance condition of the instrument, and the second stage estimator that measures the causal effect of a CDL launch on firm entry dynamics in the focal city. Panel A uses treated cities, while Panel B is restricted to Canadian cities where I include additional controls: HHI_NAICS2 and log(state-level university funding) (inflation is dropped because StatsCan does not report city-level inflation for many smaller cities). All specifications include City FE and Year FE, with robust standard error clustered at the city level.

**Panel A: Treated Cities**

| | Number of New Firms Founded | | | Log(Num New Firms) | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| treat_intensity | 65.622*** | 62.143*** | 54.988*** | 0.099*** | 0.107*** | 0.101*** |
| | (14.520) | (14.428) | (14.422) | (0.022) | (0.019) | (0.020) |
| infl_yoy | 17.750 | 16.980 | 15.684 | 0.021 | 0.017 | 0.019 |
| | (15.428) | (14.879) | (14.072) | (0.018) | (0.019) | (0.018) |
| log_pop | -185.416 | -178.927 | -171.669 | -2.737*** | -2.605*** | -2.682*** |
| | (372.694) | (368.174) | (358.578) | (0.555) | (0.652) | (0.568) |
| log_gdp | 27.481 | 31.861 | 31.290 | 0.742 | 0.648 | 0.738 |
| | (232.341) | (234.064) | (240.336) | (0.569) | (0.619) | (0.577) |
| median personal income | 0.002 | 0.002 | 0.002 | -0.000 | 0.000 | -0.000 |
| | (0.004) | (0.004) | (0.004) | (0.000) | (0.000) | (0.000) |
| UnemploymentRate_avg | 7.410 | 7.468 | 7.243 | 0.025 | 0.016 | 0.022 |
| | (8.284) | (8.239) | (8.235) | (0.015) | (0.017) | (0.015) |
| Sample | Full | Excluding Accepted | Excluding Applied | Full | Excluding Applied | Excluding Accepted |
| First-stage coeff on IV | 0.034*** | 0.034*** | 0.034*** | 0.034*** | 0.034*** | 0.034*** |
| First stage KP F-stat | 34.492 | 34.492 | 34.492 | 34.492 | 34.492 | 34.492 |
| FE: City | Yes | Yes | Yes | Yes | Yes | Yes |
| FE: Year | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 199 | 199 | 199 | 199 | 199 | 199 |
| $R^2$ | 0.104 | 0.101 | 0.085 | 0.186 | 0.154 | 0.175 |

**Panel B: Canadian Cities**

| | Number of New Firms Founded | | | Log(Num New Firms) | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| treat_intensity | 52.036*** | 49.301*** | 43.494*** | 0.269*** | 0.249*** | 0.262*** |
| | (3.116) | (3.107) | (3.100) | (0.095) | (0.086) | (0.091) |
| Sample | Full | Excluding Accepted | Excluding Applied | Full | Excluding Applied | Excluding Accepted |
| First-stage coeff on IV | 0.046*** | 0.046*** | 0.046*** | 0.046*** | 0.046*** | 0.046*** |
| First stage KP F-stat | 82.594 | 82.594 | 82.594 | 82.594 | 82.591 | 82.593 |
| FE: City | Yes | Yes | Yes | Yes | Yes | Yes |
| FE: Year | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 5317 | 5317 | 5317 | 5317 | 5295 | 5316 |
| $R^2$ | -0.297 | -0.298 | -0.297 | -0.007 | -0.007 | -0.007 |

**Panel C: Canadian Cities with Control**

| | Number of New Firms Founded | | | Log(Num New Firms) | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| treat_intensity | 45.055*** | 42.543*** | 37.173*** | 0.203*** | 0.193*** | 0.201*** |
| | (5.663) | (5.513) | (5.313) | (0.073) | (0.071) | (0.073) |
| City-Year Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Sample | Full | Excluding Accepted | Excluding Applied | Full | Excluding Applied | Excluding Accepted |
| First-stage coeff on IV | 0.048*** | 0.048*** | 0.048*** | 0.048*** | 0.048*** | 0.048*** |
| First stage KP F-stat | 62.610 | 62.610 | 62.610 | 62.610 | 62.610 | 62.610 |
| FE: City | Yes | Yes | Yes | Yes | Yes | Yes |
| FE: Year | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 672 | 672 | 672 | 672 | 672 | 672 |
| $R^2$ | -0.173 | -0.174 | -0.174 | 0.004 | 0.002 | 0.003 |

$t$ statistics in brackets

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 7: **Does CDL Stream Launch Spur Aggregate-level of Capital Supply? IV Estimation.** This table uses a time-varying academic connectivity in business-related subjects, between institutions in a focal city and the University of Toronto, as an instrument for a CDL launch in the focal city. We use the instrument for two treatment variables, where *Has_CDL*=1 after the first treatment and *Treatment_Intensity*∈[1,6] accounts for the fact that certain cities have multiple launches over the time period. Each panel reports both the first stage, which tests the relevance condition of the instrument, and the second stage estimator that measures the causal effect of a CDL launch on financing dynamics in the focal city. Panel A uses treated cities, while Panel B is restricted to Canadian cities where I include additional controls: HHI_NAICS2 and log(state-level university funding) (inflation is dropped because StatsCan does not report city-level inflation for many smaller cities). All specifications include City FE and Year FE, with robust standard error clustered at the city level.

**Panel A: Treated Cities**

| | Number of Firms Recieved Financing | | Log(Total_Amount_Raised) | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| _intensity | 59.176*** | 53.050*** | -0.115 | -0.120 |
| | (14.858) | (14.699) | (0.170) | (0.172) |
| City-Year Economic Controls | Yes | Yes | Yes | Yes |
| Sample | Full | Excluding Accepted | Full | Excluding Accepted |
| First-stage coeff on IV | 0.035*** | 0.035*** | 0.035*** | 0.035*** |
| First stage KP F-stat | 35.794 | 35.794 | 35.794 | 35.794 |
| FE: City | Yes | Yes | Yes | Yes |
| FE: Year | Yes | Yes | Yes | Yes |
| Observations | 197 | 197 | 197 | 197 |
| $R^2$ | -0.037 | -0.046 | 0.166 | 0.162 |

**Panel B: Canadian Cities with Connection**

| | Number of Firms Recieved Financing | | Log(Total_Amount_Raised) | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| treat_intensity | 52.639*** | 48.491*** | 0.176 | 0.223 |
| | (1.228) | (1.267) | (0.522) | (0.541) |
| Sample | Full | Excluding Accepted | Full | Excluding Accepted |
| First-stage coeff on IV | 0.053*** | 0.053*** | 0.052*** | 0.052*** |
| First stage KP F-stat | 163.618 | 163.618 | 120.845 | 120.845 |
| FE: City | Yes | Yes | Yes | Yes |
| FE: Year | Yes | Yes | Yes | Yes |
| Observations | 1576 | 1576 | 1628 | 1628 |
| $R^2$ | 0.609 | 0.587 | -0.001 | -0.001 |

**Panel C: Canadian Cities with Connection**

| | Number of Firms Recieved Financing | | Log(Total_Amount_Raised) | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| treat_intensity | 52.545*** | 48.198*** | -0.835* | -0.845* |
| | (3.447) | (3.341) | (0.490) | (0.483) |
| City-Year Controls | Yes | Yes | Yes | Yes |
| Sample | Full | Excluding Accepted | Full | Excluding Accepted |
| First-stage coeff on IV | 0.054*** | 0.054*** | 0.054*** | 0.054*** |
| First stage KP F-stat | 75.048 | 75.048 | 71.352 | 71.352 |
| FE: City | Yes | Yes | Yes | Yes |
| FE: Year | Yes | Yes | Yes | Yes |
| Observations | 472 | 472 | 506 | 506 |
| $R^2$ | 0.585 | 0.565 | 0.041 | 0.041 |

$t$ statistics in brackets

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 8: **Strategic Complementarity Mechanism: Local Startup Participation Intensity.** This table reports causal estimates of CDL stream launches on firm entry at the city–industry–year level, distinguishing between programs with low versus high local startup participation. For each CDL program at its initial launch, we compute the share of applicants located in the focal city and classify programs as having high local participation if this share is above the median. To ensure that the estimates are not mechanically driven by the large number of local CDL participants, the analysis excludes all CDL applicant firms from the sample.

| | Number of New Firms Founded | | Log(Num New Firms) | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| ATT (CSDID) | 7.756** | 4.667 | 0.250** | 0.155 |
| | (3.771) | (4.245) | (0.117) | (0.193) |
| Sample | High Local Share | Low Local Share | High Local Share | Low Local Share |
| City × Industry FE | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes |
| Observations | 1136 | 863 | 1136 | 863 |

$t$ statistics in brackets

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 9: **Strategic Complementarity Mechanism: Launches During COVID.** This table presents the causal estimates of CDL stream launches on firm entry at the city–industry level in a given year, for all launches during the COVID period (from 2020 to 2022), using specification 1 (Panel A) and specification 2 (Panel B). Both the full sample and the sample excluding CDL applicants and attendees are used. Standard errors are clustered by city × industry.

**Panel A: Treated Cities**

| | Number of New Firms Founded | | | Log(Num New Firms) | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| ATT (CSDID) | 2.434 | 2.276 | 2.499 | -0.304** | -0.313** | -0.328*** |
| | (3.339) | (3.321) | (3.142) | (0.142) | (0.137) | (0.125) |
| Sample | Full | Excluding Accepted | Excluding Applied | Full | Excluding Accepted | Excluding Applied |
| City × Industry FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 664 | 664 | 664 | 664 | 664 | 664 |

**Panel B: All Cities in Countries with CDL Sites**

| | Number of Firms New Firms Founded | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Treated × Posted | 2.659 | 2.698 | 2.861 |
| | (3.328) | (3.277) | (3.200) |
| Sample | Full | Excluding Accepted | Excluding Applied |
| City × Industry FE | Yes | Yes | Yes |
| Stream × Year FE | Yes | Yes | Yes |
| City × Year FE | Yes | Yes | Yes |
| Observations | 180531 | 180531 | 180531 |
| $R^2$ | 0.885 | 0.885 | 0.885 |

$t$ statistics in brackets

$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

Table 10: **Optimal Targeting Strategy: Prime vs. Non-prime Launches and Entry Effect.** This table presents the causal estimates of CDL stream launches on firm entry at the city level in a given year, for two types of launches: 1) prime, which is a generic stream that features any science-based high-tech startups; 2) non-prime, or stream-specific, which features a certain industry or technology. There are four cities (Toronto, Halifax, Calgary, Vancouver) that once launched a prime site and later launched streams. All other cities only launched streams. Since we want to estimate the city-level entry effect difference between these two types of policies, we aggregate all entries to the city-year level and divide firms into tech-focused versus non-tech-focused, with the former corresponding to "prime". A city can launch Prime only once, so we use city-year panel CSDID estimators for prime test. For stream launches, as a city can experience multiple stream launches during the time period, we utilize a Stacked-DiD estimator. Similar to previous tests, the full sample and the sample excluding CDL applicants and attendees are used. Standard errors are clustered by city × industry.

**Panel A: Prime Launch**

| | Number of New Tech firms | | | Number of New Non-tech firms | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| ATT (CSDID) | 55.750*** | 53.219*** | 49.565** | 13.521 | 14.062 | 12.878 |
| | (21.174) | (20.010) | (19.897) | (9.876) | (9.917) | (9.934) |
| Sample | Full | Excluding Accepted | Excluding Applied | Full | Excluding Accepted | Excluding Applied |
| City FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 100 | 100 | 100 | 100 | 100 | 100 |

**Panel B: Stream Launch**

| | Number of New Tech firms | | | Number of New Non-tech firms | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Treated × Post | 46.705*** | 44.258*** | 34.556*** | 1.715 | 1.854 | 2.308 |
| | (11.367) | (10.432) | (8.286) | (6.812) | (7.033) | (6.966) |
| Sample | Full | Excluding Accepted | Excluding Applied | Full | Excluding Accepted | Excluding Applied |
| City × Stack_id FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year × Stack_id FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 91 | 91 | 91 | 91 | 91 | 91 |
| $R^2$ | 0.988 | 0.988 | 0.988 | 0.963 | 0.963 | 0.963 |

$t$ statistics in brackets

$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

Table 11: **Optimal Targeting Strategy: Prime vs. Non-prime Launches and Startup Quality.** We compare startups' future performances among those founded after the prime launch versus the stream launch. Outcomes cover three dimensions: (i) failure, (ii) financing, and (iii) extreme success. Columns (1) report venture failure; Columns (3)–(5) report financing outcomes within three years of founding; Column (6) reports extreme success – whether the firm is acquired within three years of inception. We include founding-team controls (team size, prior experience, ethnicity, gender, and education). City fixed effects and founding-year fixed effects are always implemented. Variable construction is detailed in Appendix **??** and **??**. In Panels A and B, we report cross-sectional regressions for firms founded within 3 years of the prime launch and stream launch, respectively. The control group is firms that are founded before the time period and in the untreated industries (non-high-tech firms for prime launches).

**Panel A: Prime Launch**

| | Website InActive | Amt Raised | N. Rounds | N. Rounds Led by VC | N. Top Investors | Exit |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Treated Firms | -0.039*** | -0.129* | 0.156*** | 0.014*** | 0.012*** | 0.021* |
| | (0.001) | (0.045) | (0.021) | (0.001) | (0.001) | (0.007) |
| City FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 1498 | 1505 | 1505 | 1505 | 1505 | 1505 |

**Panel B: Stream Launch**

| | Website InActive | Amt Raised | N. Rounds | N. Rounds Led by VC | N. Top Investors | Exit |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Treated Firms | -0.001 | -0.144 | 0.011 | -0.002 | 0.020** | -0.017 |
| | (0.055) | (0.129) | (0.071) | (0.030) | (0.008) | (0.017) |
| Control Variables | Yes | Yes | Yes | Yes | Yes | Yes |
| City × Industry FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 8970 | 9035 | 9035 | 9035 | 9035 | 9035 |

# Appendix

## A.1 Supplementary Tables

Table A.1: Summary of Counterfactual Experiments

| Experiment | Description | Effect (p.p.) | % of Baseline |
|---|---|---|---|
| (1) No treatment | Remove CDL intervention | −0.96 | −1.3% |
| (2) Universal treatment | Extend CDL to all | +0.77 | +1.0% |
| (3) No spillovers | Set $\gamma = 0$ | −21.9 | −29.6% |
| (4) Double financing | 2× all financing | +0.45 | +0.6% |
| (5) Targeted (AI/Health/Climate) | Reallocation | −0.04 | −0.05% |

*Notes:* Effects computed relative to baseline mean entry probability of 74.1%.

Table A.2: Full Estimation Results: All Parameters

| Parameter | Estimate | Std. Error | $z$ | $p$-value |
|---|---|---|---|---|
| *Sector Fixed Effects ($\delta_s$), Base = Other* | | | | |
| $\delta_{\text{AI}}$ | 0.6773 | 0.0837 | 8.10 | 0.000*** |
| $\delta_{\text{Agriculture}}$ | −0.4364 | 0.0498 | −8.77 | 0.000*** |
| $\delta_{\text{Blockchain}}$ | −0.6442 | 0.0451 | −14.27 | 0.000*** |
| $\delta_{\text{Climate}}$ | −0.4519 | 0.0573 | −7.89 | 0.000*** |
| $\delta_{\text{Commerce}}$ | 0.0956 | 0.0997 | 0.96 | 0.338 |
| $\delta_{\text{Energy}}$ | 0.3978 | 0.0977 | 4.07 | 0.000*** |
| $\delta_{\text{Fintech}}$ | 0.4915 | 0.0876 | 5.61 | 0.000*** |
| $\delta_{\text{Health}}$ | 0.7709 | 0.0644 | 11.96 | 0.000*** |
| $\delta_{\text{Matter}}$ | −0.6479 | 0.0462 | −14.03 | 0.000*** |
| $\delta_{\text{Neuro}}$ | −0.2355 | 0.0778 | −3.03 | 0.002*** |
| $\delta_{\text{Oceans}}$ | 0.0829 | 0.0982 | 0.84 | 0.399 |
| $\delta_{\text{Other Hightech}}$ | 0.8403 | 0.0714 | 11.78 | 0.000*** |
| $\delta_{\text{Quantum}}$ | −1.6742 | 0.1171 | −14.30 | 0.000*** |
| $\delta_{\text{Risk}}$ | −0.4516 | 0.0500 | −9.04 | 0.000*** |
| $\delta_{\text{Space}}$ | −0.2632 | 0.0711 | −3.70 | 0.000*** |
| $\delta_{\text{Supply Chain}}$ | −0.1934 | 0.0895 | −2.16 | 0.031** |
| *Structural Parameters* | | | | |
| $\beta$ (log financing) | 0.0884 | 0.0288 | 3.07 | 0.002*** |
| $\gamma_{\text{cross}}$ | −0.0778 | 0.0083 | −9.37 | 0.000*** |
| $\gamma_{\text{within}}$ | 2.8475 | 0.1352 | 21.07 | 0.000*** |
| *Treatment Effects ($\tau_s$)* | | | | |
| $\tau_{\text{AI}}$ | 0.2177 | 0.0439 | 4.96 | 0.000*** |
| $\tau_{\text{Agriculture}}$ | 0.0152 | 0.0733 | 0.21 | 0.836 |
| $\tau_{\text{Blockchain}}$ | 0.1471 | 0.0696 | 2.12 | 0.034** |
| $\tau_{\text{Climate}}$ | −0.0245 | 0.0830 | −0.29 | 0.768 |
| $\tau_{\text{Commerce}}$ | 0.1228 | 0.0292 | 4.21 | 0.000*** |
| $\tau_{\text{Energy}}$ | 0.1380 | 0.0304 | 4.54 | 0.000*** |
| $\tau_{\text{Fintech}}$ | −0.2132 | 0.0932 | −2.29 | 0.022** |
| $\tau_{\text{Health}}$ | 0.3606 | 0.0353 | 10.22 | 0.000*** |
| $\tau_{\text{Matter}}$ | 0.0185 | 0.0783 | 0.24 | 0.814 |
| $\tau_{\text{Neuro}}$ | 0.1401 | 0.0545 | 2.57 | 0.010** |
| $\tau_{\text{Oceans}}$ | −0.1871 | 0.0908 | −2.06 | 0.039** |
| $\tau_{\text{Other}}$ | 0.0000 | — | — | — |
| $\tau_{\text{Other Hightech}}$ | 0.0000 | — | — | — |
| $\tau_{\text{Quantum}}$ | −0.1005 | 0.0543 | −1.85 | 0.064* |
| $\tau_{\text{Risk}}$ | −0.1048 | 0.0682 | −1.54 | 0.125 |
| $\tau_{\text{Space}}$ | 0.1178 | 0.0673 | 1.75 | 0.080* |
| $\tau_{\text{Supply Chain}}$ | 0.1332 | 0.0547 | 2.44 | 0.015** |
| Observations | 3,795 | | | |
| Log-likelihood | −1,671.69 | | | |
| Bootstrap replications | 200 | | | |

*Notes:* ***, **, * denote significance at the 1%, 5%, and 10% levels.

Table A.3: **Capital Reallocation Around CDL Stream Launch: Treated Cities Only** This table reports the causal estimates of the CDL stream launch on the competition for financing between startups. The analysis uses the full sample and the sample excluding CDL participants. We mainly look at two metrics: the number of firms received at least one round of financing in a given year around the event and the amount of funds raised by firms in a given year around the event, conditional on firm age at the time it receives funding. These proxies should depict the potential competition for funding and capital reallocation after the introduction of the incubator program for each site-stream pair. For the number of firms, we estimate effects using financing rounds with disclosed amounts. Another test using all financing rounds reveals a 3-times magnitude with the same level of significance. Standard errors are clustered by city × industry.

**Panel A: Number of Firms Received Financing**

| | Full | | | Excluding Accepted | | |
|---|---|---|---|---|---|---|
| | (1) Age 0–3 | (2) Age 4–7 | (3) Age ≥ 8 | (4) Age 0–3 | (5) Age 4–7 | (6) Age ≥ 8 |
| ATT (CSDID) | 2.353** | 1.388** | -0.863** | 1.862** | 1.172* | -0.852** |
| | (1.083) | (0.626) | (0.392) | (0.936) | (0.622) | (0.393) |
| City × Industry FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 1211 | 1211 | 1211 | 1211 | 1211 | 1211 |

**Panel B: Amount of Financing Raised**

| | Full | | | Excluding Accepted | | |
|---|---|---|---|---|---|---|
| | (1) 0To3 | (2) 3To7 | (3) Over7 | (4) 0To3 | (5) 3To7 | (6) Over7 |
| ATT (CSDID) | 1.078*** | -0.974** | 0.292 | 1.032** | -0.992** | 0.229 |
| | (0.385) | (0.461) | (0.399) | (0.427) | (0.458) | (0.395) |
| City × Industry FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 694 | 531 | 928 | 680 | 526 | 907 |

$t$ statistics in brackets

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.4: **Cross-section Test for Instrument.** This table shows how the academic connectivity in business-related subjects, between a focal city and the University of Toronto, is correlated with the likelihood of having a CDL site in the focal city. This is a cross-sectional test with no time dimension variations. *Has_Academic_Connection* is a dummy variable equal to 1 if there is some academic connectivity and serves as a proxy for the extensive margin. The other two variables measure the strength of the connection and serve as a proxy for the intensive margin. Columns (1)-(3) show results of an OLS with robust standard error, and columns (4)-(5) show OLS with country fixed effect and robust standard error clustered at the country level.

| **y = Whether a City Has CDL Site** | | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Has_Academic_Connection | 0.027*** | | | 0.027*** | | |
| | (0.008) | | | (0.001) | | |
| Connection_Strength | | 0.001** | | | 0.001*** | |
| | | (0.000) | | | (0.000) | |
| Academic_Connectivity_Instrument | | | 0.020*** | | | 0.024*** |
| | | | (0.008) | | | (0.006) |
| Country FE | No | No | No | Yes | Yes | Yes |
| Observations | 79052 | 377 | 377 | 79021 | 357 | 357 |

$t$ statistics in brackets

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.5: **Complementarity Channel: COVID Period Entry Response** This table uses a time-varying academic connectivity in business-related subjects, between institutions in a focal city and the University of Toronto, as an instrument for a CDL launch in the focal city. We use the instrument for two treatment variables, where *Has_ CDL*=1 after the first treatment and *Treatment_ Intensity*∈[1,6] accounts for the fact that certain cities have multiple launches over the time period. Each panel reports both the first stage, which tests the relevance condition of the instrument, and the second stage estimator that measures the causal effect of a CDL launch on firm entry dynamics in the focal city. Panel A uses treated cities, while Panel B is restricted to Canadian cities with academic connection. All specifications include City FE and Year FE, with robust standard error clustered at the city level.

**Panel A: Treated Cities**

| | Number of New Firms Founded | | | Log(Num New Firms) | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| COVID=1 × treat_intensity | -144.802*** | -138.545*** | -130.364*** | -0.010 | -0.002 | -0.008 |
| | (27.042) | (26.787) | (26.568) | (0.036) | (0.034) | (0.035) |
| treat_intensity | 167.598*** | 159.713*** | 146.795*** | 0.106** | 0.109*** | 0.107** |
| | (27.433) | (27.142) | (26.874) | (0.038) | (0.032) | (0.034) |
| infl_yoy | 5.435 | 5.197 | 4.597 | 0.020 | 0.017 | 0.019 |
| | (10.185) | (9.940) | (9.587) | (0.020) | (0.020) | (0.019) |
| log_pop | -65.305 | -64.005 | -63.533 | -2.728*** | -2.603*** | -2.676*** |
| | (395.566) | (390.220) | (379.834) | (0.547) | (0.645) | (0.560) |
| log_gdp | -7.543 | -1.650 | -0.242 | 0.740 | 0.647 | 0.736 |
| | (262.935) | (263.797) | (269.363) | (0.575) | (0.624) | (0.583) |
| median personal income | 0.002 | 0.002 | 0.001 | -0.000 | 0.000 | -0.000 |
| | (0.004) | (0.004) | (0.004) | (0.000) | (0.000) | (0.000) |
| UnemploymentRate_avg | 6.595 | 6.688 | 6.509 | 0.025 | 0.016 | 0.022 |
| | (10.117) | (10.013) | (9.990) | (0.015) | (0.017) | (0.015) |
| Sample | Full | Excluding Accepted | Excluding Applied | Full | Excluding Applied | Excluding Accepted |
| First-stage coeff on IV | 0.034*** | 0.034*** | 0.034*** | 0.034*** | 0.034*** | 0.034*** |
| First stage KP F-stat | 17.469 | 17.469 | 17.469 | 17.469 | 17.469 | 17.469 |
| FE: City | Yes | Yes | Yes | Yes | Yes | Yes |
| FE: Year | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 199 | 199 | 199 | 199 | 199 | 199 |
| $R^2$ | 0.085 | 0.074 | 0.043 | 0.187 | 0.154 | 0.177 |

**Panel B: Canadian Cities**

| | Number of New Firms Founded | | | Log(Num New Firms) | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| COVID=1 × treat_intensity | -209.442*** | -202.155*** | -191.405*** | -0.640* | -0.601* | -0.622* |
| | (19.769) | (19.134) | (17.895) | (0.337) | (0.315) | (0.326) |
| treat_intensity | 212.118*** | 203.814*** | 189.789*** | 0.759** | 0.708** | 0.737** |
| | (14.800) | (14.203) | (13.101) | (0.354) | (0.328) | (0.342) |
| Sample | Full | Excluding Accepted | Excluding Applied | Full | Excluding Applied | Excluding Accepted |
| First-stage coeff on IV | 0.046*** | 0.046*** | 0.046*** | 0.046*** | 0.046*** | 0.046*** |
| First stage KP F-stat | 13.667 | 13.667 | 13.667 | 13.667 | 13.666 | 13.667 |
| FE: City | Yes | Yes | Yes | Yes | Yes | Yes |
| FE: Year | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 5317 | 5317 | 5317 | 5317 | 5295 | 5316 |
| $R^2$ | -0.016 | -0.029 | -0.057 | -0.010 | -0.010 | -0.010 |

**Panel C: Canadian Cities with Control**

| | Number of New Firms Founded | | | Log(Num New Firms) | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| COVID=1 × treat_intensity | -195.972*** | -189.033*** | -179.099*** | -0.295* | -0.264 | -0.283* |
| | (21.482) | (20.840) | (19.705) | (0.171) | (0.160) | (0.166) |
| treat_intensity | 198.502*** | 190.557*** | 177.408*** | 0.434** | 0.400** | 0.423** |
| | (19.484) | (18.852) | (17.843) | (0.196) | (0.185) | (0.192) |
| City-Year Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Sample | Full | Excluding Accepted | Excluding Applied | Full | Excluding Applied | Excluding Accepted |
| First-stage coeff on IV | 0.048*** | 0.048*** | 0.048*** | 0.048*** | 0.048*** | 0.048*** |
| First stage KP F-stat | 12.465 | 12.465 | 12.465 | 12.465 | 12.465 | 12.465 |
| FE: City | Yes | Yes | Yes | Yes | Yes | Yes |
| FE: Year | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 672 | 672 | 672 | 672 | 672 | 672 |
| $R^2$ | 0.065 | 0.054 | 0.027 | -0.004 | -0.005 | -0.004 |

$t$ statistics in brackets

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.6: **New Firm Performance After CDL Launch: Cross-sectional Test**.We compare startups founded after the CDL launch of a targeted stream with those founded before within the same city–industry cells. We restricted the sample to firms founded between 2010 and 2019. Outcomes cover three dimensions: (i) failure, (ii) financing, and (iii) extreme success. Columns (1)–(3) report venture failure; Columns (4)–(7) report, conditional on receiving financing, financing outcomes within three years of founding; Column (8)–(10) report extreme success – whether the firm is acquired within three years of inception, and, conditional on acquisition, the acquisition price. In Panel A, we report cross-sectional regressions without controls. In panel B, we include founding-team controls (team size, prior experience, ethnicity, gender, and education). City × industry fixed effects and founding-year fixed effects are always implemented. Variable construction is detailed in Appendix **??** and **??**. The treatment labeling follows the procedure used in the aggregate regressions. Standard errors are clustered at the city × industry level.

| | **Closed in CB** | **Website Inactive** | **Had Investor Financing** | **Amt Raised** | **N. Rounds** | **N. Rounds Led by VC** | **N. Top Investors** | **Financed in Past 2 Years** | **Acquired** | **Acq Price** |
|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| New Firms | 0.042*** | 0.033 | -0.047* | -0.675*** | 0.081 | -0.122*** | 0.023 | -0.049 | 0.165** | -0.037 |
| | (0.011) | (0.025) | (0.027) | (0.253) | (0.158) | (0.031) | (0.015) | (0.042) | (0.084) | (1.036) |
| City × Industry FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Founding Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 214833 | 211445 | 214833 | 54529 | 54532 | 54532 | 54532 | 93006 | 12603 | 969 |
| Control Variables | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

$t$ statistics in brackets

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

## A.2 Supplementary Figures

Figure A.1: **U of T BSchool Academic Connection to Business Institutions Around the World.** This figure unpacks the sources of our instrumental variables. This figure shows business school paper collaboration between the University of Toronto and other institutions worldwide. The five most frequently occurring countries in the IV dataset are shown. The top four—the United States, Canada, the United Kingdom, and France, which, unsurprisingly, correspond to the focal countries in which a CDL site was launched during the sample period.

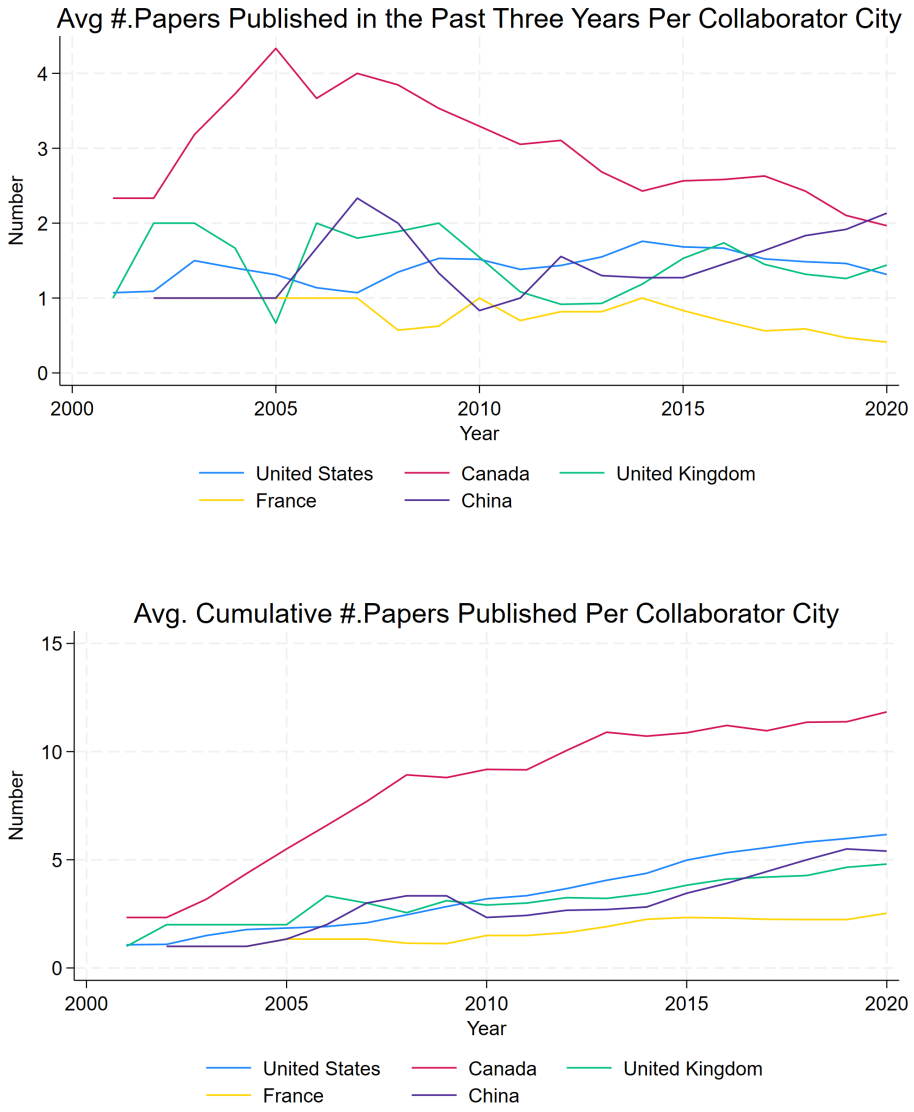
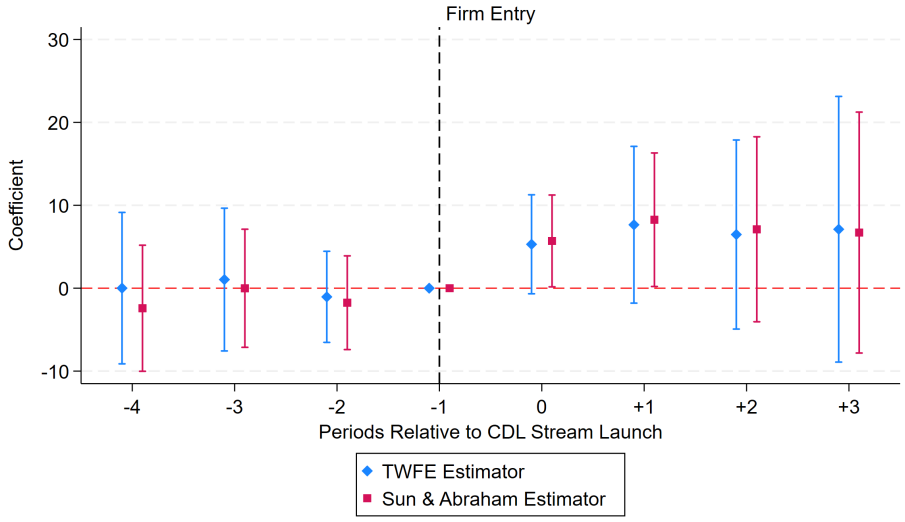
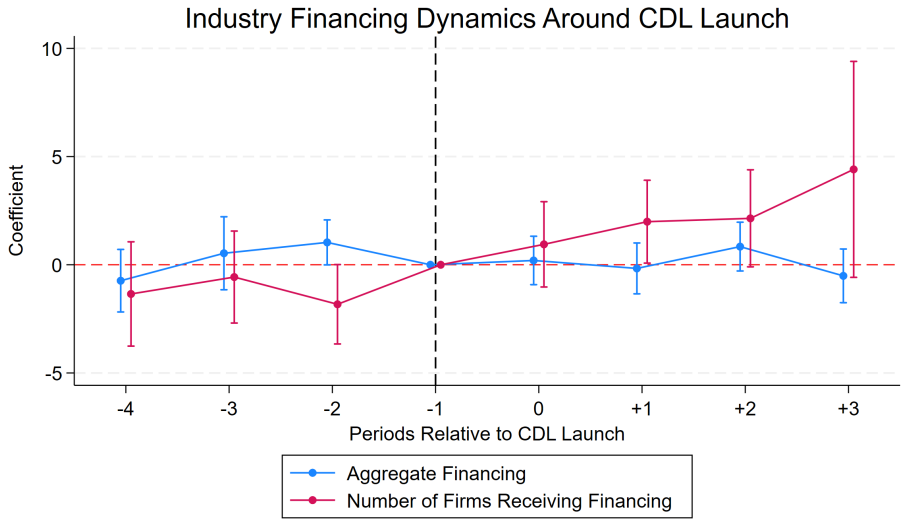


Figure A.2: **City-Industry Market Dynamics Around New CDL Stream Launch: Including Never Treated Cities.** This figure shows the dynamics of firm entry and the amount of financing attracted around a new stream launch by CDL. We estimate a triple DID with high-level dimension fixed effects to control for potential confounding factors by following $\theta_t$ of equation $Y_{i,c,t} = \sum_{t \neq -1} \theta_t Periods_t * Launch_{i,c,t} + \alpha_{i,c} + \alpha_{i,t} + \alpha_{c,t} + \epsilon_{i,c,t}$. The estimates compare differences in firm entry and financing between the focal stream and other streams in the focal city with those in other cities around the stream launch year. To allow for an adequate post-period, we restrict our analysis to launches before 2020. Panel (a) shows city-industry firm entry dynamics; Panel (b) shows industry-city financing dynamics and counts firms with non-missing financing rounds. The specification includes city × industry FEs, city × year FEs, and industry × year FEs. The year before the event is omitted as the base year. Standard errors are clustered by city × industry.




(a) Firm Entry




(b) Financing

## A.3 Firm-Level Variable Construction

This appendix details the construction of the primary variables used in the analysis, derived from the raw data files (`organizations.csv`, `people.csv`, `degrees.csv`, and `jobs.csv`).

**Sample Filtering** Filtered to include only firms founded between 2000 and 2023. Founder and job samples are restricted to individuals associated with the filtered firms.

**Founding Team and Executive Identification** Founders and executives are identified by applying regular expression (regex) patterns to job titles. Then we classify an individual as a founder if the cleaned title contains the pattern `founder`, `cofounder`, or `founding` (regex-based matching). Next, we construct five executive flags from the cleaned title (e.g., contains `chief`+`officer`, `executive`+`officer`, any C.O title such as CEO/CFO/CTO, or phrases such as `executive director`). We also flag non-executive board-type titles (e.g., `non-executive`, `executive committee`) and subtract these from the executive count. Let `ExecSum` denote the resulting net executive score; then

$$\texttt{Executive} = \mathbb{1}\{\texttt{ExecSum} > 0\}.$$

For job records, we also set `Executive` to one when `job_type` is `executive`.

**Demographic Proxy Construction** Because gender and ethnicity are often missing in raw data, they are proxied using name-based algorithms:

- *Gender*: First names are processed through the `gender_guesser` package. Results are mapped to "male" or "female," while "mostly_male" and "mostly_female" are collapsed into their respective categories.
- *Ethnicity*: Full names are processed using the `ethnicolr2` package to predict ethnic groups based on the highest probability assigned to the name parts.

**Education Level and Specialization** Degrees are mapped to a five-point ordinal scale: `PhD`, `Masters`, `Bachelor`, `College`, and `Other`. The degree `subject` field is classified via keyword matching:

*Science (STEM)*

```
'biolog', 'chemi', 'physi', 'math', 'statistics', 'auto', 'robotics',
'tronics', 'electric', 'quant', 'neuro', 'biochemistry', 'neuro', 'cognitive',
'zoology', 'genetics', 'earth', 'environment', 'ecology', 'dna', 'photonics',
'medic', 'geology', 'astronomy', 'biomedical', 'biophysics', 'engineering',
'computer', 'mechanic', 'civil', 'software', 'robotics', 'systems',
'technology', 'informatics', 'network', 'ai', 'data', 'computer', 'aero',
'defense', 'geomatics', 'system', 'cyber', 'telecom', 'iot', 'machine
learning', 'artificial intellilgence', 'medicine', 'psychology', 'clinical',
'biomedical', 'pharmacy', 'nutrition', 'denti', 'olog', 'disease', 'anatomy',
'health', 'pathology', 'neurobiology', 'physi', 'natural science', 'nano',
'traffic', 'ee', 'ece', 'comput', 'cogsci', 'medical'.
```

*Business*

```
'business', 'commerce', 'marketing', 'management', 'finance', 'accounting',
'strategy', 'administration', 'entrepreneur', 'supply chain', 'leadership',
'analytics', 'economics', 'mba', 'corporate', 'econ', 'executive',
'entrepreneurship', 'vc', 'venture capital', 'pe', 'm&a', 'hr'.
```

Degree information is recorded at the founder–firm level and may vary over time. For each founder-firm pair, we retain the highest degree attained as of the firm's founding date. In addition, we construct indicators for whether the founder has ever obtained a business degree or a science degree, regardless of whether it is associated with the highest degree.

**Experience and Career History** Prior experience is calculated by aggregating job history relative to the firm's founding date. `Has Experience Before` is a binary indicator for any job record prior to the current founding role. `Entrepreneurial Experience` is a binary indicator for whether the individual has previously served as a founder and the cumulative count of previous founding roles.

**Geographic Proximity to CDL Sites** Distances are calculated using the `geopy` library. Latitude and longitude for cities are retrieved via the `Nominatim` geocoder. Geodesic distance (in kilometers) is calculated between the firm location and nine CDL benchmark sites: Toronto, Vancouver, Montreal, Calgary, Halifax, Oxford, Paris, Atlanta, and Wisconsin. `Closest_CDL_Site` identifies the nearest CDL location, and `Closest_CDL_Distance` records that minimum distance.

**Website Validation** To determine firm activity, homepage URLs are probed asynchronously using the `requests` library. A site is marked as active if a HEAD or GET request returns a status code $< 400$. The `Last-Modified` header is extracted where available to proxy the date of the firm's last digital update.

**Stream Mapping Keywords** An industry tag is assigned to a CDL stream if the stream name or description matches that of a stream in the program list. If no direct match is found, the following semantic keyword map is applied. Each stream is associated with the corresponding set of keywords:

- *AI:* artificial intelligence; machine learning; computer vision; robotics; automation; ai
- *Health:* health; biotechnology; pharmaceutical; medical; life science; digital health
- *Blockchain:* blockchain; cryptocurrency; ethereum; bitcoin; distributed ledger; web3
- *Energy:* energy; battery; renewable energy; oil and gas; electric vehicle; energy storage
- *Space:* space; aerospace; satellite; satellite communication
- *Quantum:* quantum; quantum computing
- *Supply Chain:* supply chain; logistics; warehousing; shipping
- *Oceans:* marine; oceans; aquaculture
- *Climate:* cleantech; green; sustainability; environmental engineering; climate
- *Agriculture:* agriculture; farming; agtech; horticulture
- *Fintech:* fintech; payments; insurance; banking; financial services; wealth management; insurtech
- *Neuro:* neuro; neuroscience; brain; mental health

An industry tag is classified as `tech` if it satisfies either of the following conditions: (i) it contains any of the long-form technology phrases listed below (substring matching), or (ii) it contains any of the listed acronyms as a standalone word (exact word-boundary matching).

*Technology phrases:* "technology", "cloud computing", "cloud infrastructure", "cloud security", "saas", "paas", "machine learning", "artificial intelligence", "blockchain", "cryptocurrency", "web3", "quantum computing", "autonomous vehicles", "electric vehicle", "augmented reality", "virtual reality", "robotics", "drones", "wearables", "satellite communication", "big data", "analytics", "predictive analytics", "cyber security", "biotechnology",

"digital health", "health diagnostics", "telemedicine", "renewable energy", "energy storage", "smart home", "smart cities", "cleantech", "fintech", "insurtech", "digital banking", "mobile apps", "developer apis", "gaming", "social media marketing", "online games", "digital media", "content delivery network", "e-commerce platforms", "digital entertainment", "automation", "automative"

*Acronyms (whole-word matching):* "ai", "ar", "vr", "ev", "iot", "5g"

An industry tag is classified as "other" if it matches none of the above keywords and doesn't belong to any of the steam.

**IPOs and Acquisitions** While IPOs and acquisitions represent canonical "successful" exits, many transactions do not disclose sales price—effectively low-value exits. Consistent with prior work, we treat no-price deals as low-price sales by imputing a conservative 1× Multiple on Invested Capital (MOIC), which implies zero net gain to investors. We merge exit records from Crunchbase and Refinitiv SDC Platinum M&A, assign 1× MOIC where prices are missing, and then apply MOIC thresholds, from 1× to 4×, to classify exits as successful (MOIC $\geq$ threshold) or unsuccessful (MOIC $<$ threshold).

**Interim Success** For firms not yet exiting, we adopt early-stage success proxies. These include total funding raised, number of funding rounds, number of funding rounds led by VC investors, backing by a top-tier investor within three years of founding, as well as securing a follow-up round within two years.

**Failure** We use Crunchbase's operating status-specifically, firms listed as inactive or closed-as our primary failure indicator. We then supplement this with web-traffic data: any company whose website remains persistently inactive or exhibits chronically low traffic is likewise classified as a failure.

Table A.7: Journals We Used to Construct the Instrument, by Field

| **Field and Journal Name** | |
|---|---|
| **Management, Strategy, & Organization** | |
| • Academy of Management Journal | • Strategic Organization |
| • Academy of Management Review | • Global Strategy Journal |
| • Academy of Management Perspectives | • Strategic Entrepreneurship Journal |
| • Academy of Management Annals | • Long Range Planning |
| • Administrative Science Quarterly | • Journal of Management |
| • Harvard Business Review | • Management Science |
| • MIT Sloan Management Review | • Organization Science |
| • California Management Review | • Organizational Dynamics |
| • Strategic Management Journal | • Human Relations |
| **Economics (General, Applied, & Law)** | |

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*Table A.7, continued from previous page*

**Field and Journal Name**

- American Economic Review
- Quarterly Journal of Economics
- Journal of Political Economy
- Econometrica
- Review of Economic Studies
- Journal of Economic Theory
- Economic Journal
- European Economic Review
- Journal of Monetary Economics
- Journal of Public Economics
- Journal of Labor Economics
- Journal of International Economics
- Journal of Development Economics
- Journal of Urban Economics
- Journal of Health Economics
- Journal of Environmental Economics
- RAND Journal of Economics
- Int. Journal of Industrial Org.
- Journal of Industrial Economics
- Review of Industrial Organization
- J. of Economics & Management Strategy
- Industrial and Corporate Change
- Information Economics and Policy
- Int. J. of the Economics of Business
- J. of Competition Law & Economics
- Antitrust Bulletin
- Competition Policy International
- World Competition
- Journal of Law and Economics
- Journal of Legal Studies
- Int. Review of Law and Economics
- Journal of Applied Econometrics
- Economic Inquiry
- Economics Letters
- Applied Economics
- Empirical Economics

**Finance, Marketing, & Operations**

*Continued on next page...*

*Table A.7, continued from previous page*

| **Field and Journal Name** |
|---|
| • Journal of Finance<br>• Journal of Financial Economics<br>• Review of Financial Studies<br>• J. of Financial & Quantitative Analysis<br>• Financial Management<br>• Journal of Corporate Finance<br>• Journal of Banking & Finance<br>• Journal of Financial Intermediation<br>• Review of Finance<br>• Financial Analysts Journal<br>• Journal of Portfolio Management<br>• Journal of Financial Markets<br>• Journal of Empirical Finance<br>• Journal of Marketing<br>• Journal of Marketing Research<br>• Marketing Science<br>• Journal of Consumer Research<br>• Journal of Consumer Psychology<br>• Int. Journal of Research in Marketing<br>• J. of the Academy of Marketing Science<br>• Journal of Advertising<br>• Journal of Retailing<br>• Industrial Marketing Management<br>• Journal of Operations Management<br>• Production and Operations Management<br>• Manufacturing & Service Ops Mgmt<br>• Operations Research<br>• European J. of Operational Research<br>• Int. J. of Production Economics<br>• Supply Chain Management<br>• Decision Sciences |
| **Specialized Fields (Innovation, IS, Ethics, & HR)** |

*Continued on next page...*

*Table A.7, continued from previous page*

| **Field and Journal Name** |
|---|
| • Entrepreneurship Theory and Practice<br>• Journal of Business Venturing<br>• Small Business Economics<br>• International Small Business Journal<br>• Entrepreneurship & Regional Development<br>• Research Policy<br>• Technovation<br>• Journal of Int. Business Studies<br>• Journal of World Business<br>• International Business Review<br>• Management International Review<br>• Journal of Organizational Behavior<br>• OB and Human Decision Processes<br>• Leadership Quarterly<br>• Journal of Applied Psychology<br>• Personnel Psychology<br>• Human Resource Management<br>• Work, Employment and Society<br>• MIS Quarterly<br>• Information Systems Research<br>• Journal of Management Info Systems<br>• European Journal of Info Systems<br>• Journal of Business Ethics<br>• Business Ethics Quarterly<br>• Business Strategy and the Environment<br>• Journal of Business Research |